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THE EVOLUTIONARY CONTROL OF ORGANISMS AND ITS SIGNIFICANCE[1]

I

A COMPARATIVELY brief period has passed since the evidence brought together by Darwin in connection with the results slowly accumulated from other sources has clearly demonstrated that the diversity of organic life in the world occurs through evolution. It is one thing, however, to clearly diagnose a condition and quite another to understand the causes which have brought about the phenomenon so that similar results may be produced advantageously. With the assumption that evolution was merely the survival of those forms which were best adapted to the environment, generation after generation, the explanation of the method as well as its practical application, namely the improvement of organisms in any given direction, was apparently a simple matter. It seemed evident that man had modified and adapted to his welfare various plants and animals by a more or less unconscious and haphazard selection long before history records civilization.[2] Why then could not civilized man carry forward the work and with the knowledge gained since the principles of evolution were recognized, obtain far-reaching results within a brief period of time. All that seemed necessary was to have individuals

[1] Presidential address before the twenty-third annual meeting of the Ohio Academy of Science, Oberlin, O., November 28, 1913.

[2] One need not be a pessimist to assert the actual evidence thus far obtained indicates that the supposed progress made in the improvement of domesticated animals and plants is nothing more than the sorting out of pure lines and thus represents no advancement.

of a particular organism in large numbers, and by continued selection of the variations best meeting the conditions move rapidly forward by a series of increments toward the goal of perfection. What could be more simple? Instead of corn having an acreage yield of fifty bushels, there would with a proper supply of plant food be a production of two hundred, two hundred and fifty or even three hundred bushels. Instead of politicians with no perspective beyond their immediate welfare—a reelection—instead of college presidents and faculties with their numerous shortcomings —according to the students and occasionally the trustees—there would be the ideal individual bred to specification and not necessarily made in Germany.

Unfortunately, variations with a perverseness incomprehensible uniformly refused to accumulate in the manner desired and at times even demonstrated their obstinacy by retrogression. It was plainly evident that there were limits imposed by nature not easily passed, and in connection with which much experimental work must be undertaken before definite progress was made and the facts fully understood.

With a realization of the difficulties involved in an attempt to apply evolution, it will be well to pause for a moment and consider certain fundamental principles before discussing the results of some of the investigations which for a time at least promised much toward the solution of the problem. Thus it may be stated that evolution in its different modifications postulates in general (1) the occurrence of numerous varying individuals, some of which are (2) eliminated by environmental stimuli leaving few or no offspring, while (3) the survivors transmit to their progeny the characters which proved of selective value, with the result that (4) through the continuation of the process the race eventually becomes adapted to surrounding conditions. The first two propositions are merely statements of fact. The real difficulties of the situation are those of ascertaining how variations which are transmitted may be recognized and produced so that the result will be a cumulative one. Until this is done breeders must continue to proceed in the same haphazard manner that they have followed for countless generations.

By selecting the largest and most perfect ears of seed corn from the variations present in the field, conversely eliminating the remainder from reproducing, the corn grower plants with a fatuous trust in providence that a crop somewhat better or at least as good as the preceding crop will be produced. If it is a type comparatively pure the average may be maintained and the hope partially realized, but the chances for retrogression are far greater than for advancement, inasmuch as there is no means for distinguishing a variation which will be transmitted with equal or better results than in the preceding generation, from one that represents a fluctuation due to nurture and which is non-transmissible. Thus the apparently inferior ear of corn will frequently produce a yield far better than obtained from one which is perfection as graded by the methods of the "corn show," and if from the same pure race, the resultant crop will be at least as good. Artificial methods of hybridization, which furnish an immediate advancement in the succeeding generation, result in a gain which is only temporary. The increased stimulus to growth vanishes as a fluctuation.

Thus it is quite evident that there exists a problem in the evolutionary control of organisms even the partial solution of which will mark an extraordinary advancement not only for agriculture, horticulture,

and animal breeding, but also for society in general.

II

The general results of the investigations bearing upon the evolutionary control of organisms may be grouped around the principles of Mendelism, the mutation theory, and pure line breeding.

The rediscovery in 1900 of the fundamental laws governing hybridization so brilliantly established by Mendel in 1865, but unfortunately concealed in the obscure publications of the Natural History Society of Brunn, opened an extraordinary field for experimental work. This has already developed to vast proportions in connection with both the results obtained and the speculations involved, while the end is not in sight.

The investigations of Mendel, now so familiar to all biologists, and which may be mentioned somewhat in detail here because of their bearing on mutation, consisted primarily in the crossing of tall and dwarf peas, with the result that the first filial (F_1) or hybrid generation consisted entirely of tall plants. When, however, seeds from these plants were sown the ratio of tall to dwarf plants became 3 to 1 in the second (F_2) hybrid generation, a result explained by the theory of dominant and recessive characters on the basis that there are certain determiners of unit characters in the germplasm which dominate over others during the development of the somatoplasm or body of the individual in the higher forms of life. More recently the presence and absence theory has been applied in interpreting the results. In a manner similar to the preceding when smooth yellow peas were crossed with wrinkled green peas the first hybrid generation consisted of smooth yellow forms inasmuch as the character smooth and the character yellow were dominant over the character wrinkled and the character green, and the crosses were known as dihybrids, inasmuch as they differed in respect to two characters. In the second hybrid generation the resultant ratio was 15 to 1 pure recessive, *i. e.*, wrinkled green, although the fifteen consisted of smooth yellow, smooth green, and wrinkled yellow in the proportion of 9 : 3 : 3. In the same way trihybrids have the ratio 63 to 1 pure recessive while any polyhybrid differing in "n" characters which mendelize in the usual manner will give an expected ratio of $4^n - 1$ to 1 pure recessive, which will become apparent only through the breeding of large numbers of individuals.

While the preceding summary represents the normal results in connection with the segregation of unit characters, studies of the past few years have demonstrated that many interesting relationships may occur between the factors governing the production of characters. For example, it has been found that two or more determiners are often present either of which will produce the given character as Nillson-Ehle demonstrated in hybrids of brown and white chaffed wheat, while on the other hand two or more determiners acting together may be necessary to bring about an effect. Such a condition exists, as Bateson in 1910 showed, in certain white-flowered sweet peas which when crossed produce purple flowers in the first hybrid generation. The results which have led to the theory of coupling and of repulsion, particularly the latter, where the expectancy of a pure recessive may be one among many thousands, go far toward suggesting a possible explanation of many so-called mutations on the basis of ancestral individuals heterozygous for one or more characters.

Do the Mendelian principles assist us, however, in attaining the goal which we are seeking, namely the building up of an ideal

organism which will continue to transmit its characters? The answer must be in the negative so far as the originating of anything new is actually concerned. Recessives may be obtained. Characters may be redistributed. They were present in the forms first utilized, however.

The mutation theory formulated by De Vries in 1901 approximately at the time interest was being awakened by the rediscovery of the hybridization principles of Mendel, needs no extended explanation to those who have been interested in evolution. Based on cultural experiments with *Œnothera lamarckiana,* one of the evening primroses, the appearance of relatively small numbers of forms which were quite distinct from the parental species and which bred true in subsequent generations, led to the inference that evolution had in many cases proceeded by discontinuous variations or mutations.

Long series of breeding experiments followed in connection with other organisms, both plants and animals, with results quite similar to those obtained by De Vries. Investigations were also made (Fischer, MacDougal, Tower, etc.) where organisms were subjected to stimuli abnormal in their nature, with the result that a modified progeny was obtained which bred true to the apparently induced character in succeeding generations. Furthermore, cytological studies (Gates, etc.) demonstrated some interesting relationships so far as differ- "mutants" were concerned.

While the evidence is far too insufficient to allow more than a tentative opinion, there are several conclusions concerning mutation which appear justified. The nature of the results obtained through the various agencies make it quite evident that they are not all due to a single underlying principle. There are many "mutants" the origin of which is most certainly to be explained on the basis of a heterozygous condition of the gametes, and much evidence has accumulated that *O. lamarckiana* of De Vries on which the mutation theory was founded belongs to this class. Furthermore there are mutants developing in connection with the action of abnormal stimuli although it is not at all improbable that some of these result from heterozygotes. It may be mentioned that Humbert (1911) in experiments with 7,500 pure line plants of *Silene noctiflora,* one of the "pinks" utilizing methods similar to those of MacDougal, failed to obtain any "mutants." Another explanation of the results in connection with the influence of abnormal stimuli is that the modification takes place through the destruction of a factor and thus the process is one of subtraction instead of addition. There are also investigations, notably those of Gates, in which the aberrant organism apparently results from the abnormal behavior of the chromosomes at some stage during the life cycle. *Œnothera gigas* with its tetraploid chromosomes is here of much interest.

Notwithstanding these diverse results, there is little indication that anything actually new has been added to the organism which would not have occurred within a pure line. If this is true the heterogeneous school of mutationists can be of little assistance beyond suggesting the way in which evolution did not take place.

The experiments on the basis of pure line breeding belong to a comparatively recent period and are of the utmost importance. Johannsen in 1903 published results based on a pure line of beans self-fertilized for successive generations and evidently homozygous. From a bean weighing 95 centigrams and far above the average in size he obtained plants producing beans varying in weight from approximately 35 to 70 centigrams, but all far below the weight of the parent. Utilizing

these in turn as parental forms, from those having a weight of 35–40 centigrams there resulted a progeny with an average of 57.2 centigrams, while from those having a weight of 65–70 centigrams a progeny was obtained which had an average of 55.5 centigrams. In other words, selection had not only failed to make any advancement, but actually resulted in a slight retrogression. Facts quite in accord with this but giving much more pronounced results have been obtained by Tower (1906), Jennings (1908), Johannsen (1909) and others. It should be noted, however, that there have been several experiments, notably those of De Vries with buttercups, Tower with potato beetles, and Smith with Indian corn, where a possible advance of a character was recorded in a group. Heterozygotes here may have been responsible for the result, although again the explanation may consist in the elimination of the effects of a determiner.

The results in mixed races as exemplified by corn, beans, etc., where selection has gradually improved a group of organisms but finally reached a limit beyond which no progress appeared possible, are comparatively well understood and are due, as explained by Shull (1908), to the separation of the pure lines which were present in the race at the beginning. This is where the average agriculturist, horticulturist, and animal breeder has gone far astray and, having succeeded for a few generations in making progress, has failed to understand why he may not continue to be successful.

Thus we find that attempts to modify a character by selection within pure lines within a small number of generations have almost universally failed, and that the few apparent results to the contrary must be looked upon with the suspicion that the population was a mixed race and that Mendelian principles applied.

Once again we are led to propound with still greater emphasis the question, "How then has evolution taken place?" "In what manner have organisms acquired their characters?" "Is it possible to escape the difficulties that confront the investigator on every side?"

III

The application of statistical methods to problems of biology has provided and will continue to provide facts of decided value obtainable in no other way. Nevertheless, the use of data "en masse" uncoordinated with experimental methods can not solve the riddle of existence so easily as some, at an earlier period at least, would have had us believe. There are, however, investigations which seem fundamental to the problem under discussion and which may well be approached from the statistical side. These relate to the influence of certain factors composing the environment as well as to the part played by asexual and sexual reproduction, corresponding in reality to close and cross breeding, upon variability and size in organisms.

Some studies undertaken in 1900 in connection with the influence of food supply on variability[3] based upon the comparison of groups of *Chrysanthemum leucanthemum* L., the common white daisy, as well as *Perca flavescens* Mitch., the yellow perch, indicated that the difference in variability as evinced by the coefficient of variation for a group with a maximum food supply as compared with a group having a minimum food supply, was extremely small and well within the limits allowed by the probable error. From this the inference was that external stimuli played an extremely unimportant part under normal conditions as a cause producing variability in general.

Attempts were subsequently made to ob-

[3] SCIENCE, p. 728, 1907.

tain data bearing on the results of close breeding and cross breeding which differ merely in degree from parthenogenesis and amphimixis. The question is an important one, for if cross breeding is only valuable in sorting out and combining existing characters, it not only obscures the facts, a knowledge of which is necessary before progress can be made in building up new characters, but results in no actual advancement in cumulative evolution. Here the material for study consisted of scalariform or cross-bred and lateral or close-bred (parthenogenetic) zygospores—in reality the young individuals—of the common filamentous green alga *Spirogyra inflata* (Vauch). Upon applying statistical methods the close-bred zygospores were found to be 23 per cent. more variable[4] in size as well as larger, both in length and actual volume, than the cross-bred zygospores. The results were not in accord with the general belief that cross breeding increased variability, although studies by Warren, Kellogg, Casteel and Phillips had pointed out that this belief was not substantiated by facts, which, however, did not actually warrant the idea that variability was decreased in cross-bred forms. The studies on the zygospores also suggested that sex existed primarily for the purpose of limiting variability, a hypothesis proposed on purely theoretical grounds by Hatschek in 1887. Another conclusion which followed from the same investigation was that in connection with the origin of death[5] and which may be mentioned here. This is summarized by stating that death apparently occurs as the result of the continually forming body cells becoming so variable through absence of control by amphimixis, that eventually some one group of functional importance fails to meet the limits imposed by the environment. In consequence of this the group, together with the remainder of the colony—the individual—perishes.

In connection with the difference in the variability of close-bred and cross-bred zygospores it seems quite evident that the result is brought about by some factor other than the environmental stimuli which are assumed to produce fluctuation, inasmuch as the material was homogeneous in every respect with the exception of the manner of reproduction. The question is a difficult one, however, not to be settled by a single investigation giving positive results, and because of its importance should receive attention.

In reference to those who hold to the belief that cross breeding, conjugation and amphimixis—the three terms differ merely in degree—increase variability, it may be well to inquire concerning some of the evidence which has been instrumental in formulating the opinion. Without any desire to be critical and at some risk of exceeding the controversial bounds which a paper of this nature allows, a few of the more important investigations touching upon the subject will be considered.

Castle, Carpenter, Clark, Mast and Barrows (1906) in a series of observations as to the effect of cross breeding and close breeding on the variability and fertility of the small fruit fly, *Drosophila ampelophila* Loew., stated that "inbreeding did not affect the variability in the number of teeth on the sex comb of the male, nor the variability in size," basing the opinion on the coefficient of variation in the number of spines and the standard deviation in the length of the tibia. In the former case the data certainly do not permit a clear conclusion one way or the other, but the value of the character which represents the sum of the teeth of the sex combs of the right

[4] Science, p. 907, 1908.

[5] Science, p. 935, 1912.

and left proximal tarsal segment, where there is undoubtedly correlation, may be open to objection under any consideration. If, however, from the data presented in the study the value of the coefficient of variation is computed, which, strange to say, was not done in the paper, and thus allowance made for the greater length of tibia in the cross-bred forms, the combined inbred forms exhibit a variability relatively 68 per cent. greater than the cross-bred forms.

Jennings (1911) in summarizing breeding experiments with *Paramecium* concluded that "The progeny of conjugants are more variable in size and in certain other respects than the progeny of the equivalent non-conjugants," and farther, "Thus conjugation increases variation." Continuing the investigations, he subsequently stated (1913) that conjugation increased the variation in the rate of reproduction. While the careful methods used by Jennings have brought to light many interesting and valuable facts, it is evident, from a critical consideration of the data, that they by no means allow such conclusions.

So far as size is concerned in a pure race, non-conjugants and their progeny were more variable than conjugants and their progeny, as noted in Table No. 28. In a wild race the progeny of the conjugants were slightly more variable than the progeny of the non-conjugants, as illustrated in Table No. 32, although in two of the nine generations tabulated the variability was greater in the case of the non-conjugants. So far as the rate of fission is concerned, the evidence is unmistakable that the conjugants were more variable. There is, however, a comparatively simple explanation for this when the statement is noted that the number of abnormal individuals, as well as the mortality, was greatest among the progeny of the conjugants. With a considerable number of forms thus having a lower rate of fission, one could expect nothing except a greater variability in the rate of fission. This becomes the more evident when it is found that the higher variability of the conjugants was caused by the considerable number with the low rate of fission.

Considering the data obtained in the breeding of plant forms where the assumption has long been prevalent that hybridization increases variability, it is found that the variability of the F_2 generation as compared with the F_1 generation or a single parental generation may be increased, but that the actual variability as a whole is not increased when the united parental types are taken into account. This may be illustrated by utilizing data from an interesting paper by Hayes (1912) dealing with correlation and inheritance in tobacco. Here, calculating the constants for two parental types combined (401 and 403) in respect to number of leaves and height of plant, it is found that the coefficient of variation has decidedly decreased through the hybridization, although the number of combinations have increased.

There exists the possibility, however, that variability will appear to be increased when forms having the same phenotype but different genotypes are bred together. Such a condition may be illustrated by the two white strains of sweet peas crossed by Bateson which produced purple flowers in the first (F_1) hybrid generation, and purple, pink, mixed, and white flowers in the second (F_2) hybrid generation. New combinations occur, but there is no evidence of increase in unit characters, nor is there an actual increase in variability.

Turning for a moment to size characters, the influence of cross breeding or conjugation is of decided interest inasmuch as facts bearing on the solution of the problem as to how size may be increased to the phys-

iological limit, even though the results hold for a single generation, have the greatest practical value for the future of agriculture and animal breeding.

It should first be noted that size in a unicellular organism is dependent on the absolute size of the individual cell with a limit undoubtedly imposed by laws governing the ratio between volume and surface in connection with osmosis. In multicellular organisms, however, size characters may depend upon either the size or the number of the component cells or upon both factors. This distinction possibly explains an apparent diversity in results obtained in the two groups.

Darwin, Mendel and others who have seriously considered the question have recognized that hybrids, among plant forms in particular, usually grew to a larger size than either parental form, a result probably due to the increased rapidity of cell division and consequently greater number of cells as conjectured by East. In the study of zygospores of *Spirogyra* it was therefore noticed with some interest that the cross-bred forms were smaller than the close-bred forms so far as both length and volume were concerned. Jennings (1911) in his study of *Paramecium* reached a contrary conclusion, stating that "The progeny of conjugants . . . were a little larger than the progeny of non-conjugants and the difference appears to be significant." This is correct merely in reference to length, however, and that it is not true for actual size as indicated by volume is evident on applying the formula for the volume of a prolate spheroid ($V = 1/6\pi ld^2$) by which it may be demonstrated that the non-conjugant forms, while smaller than the others at the beginning of the experiment, actually became larger. Thus in agreement with the zygospores of *Spirogyra*, conjugation decreased size.

The question immediately occurs as to the cause of the increased size and vigor among cross-bred multicellular organisms when the evidence indicates that cross-bred unicellular forms are smaller instead of larger. Some investigations that I have undertaken indicate an answer apparently meeting the conditions. While sufficient control experiments have not been made to venture more than a provisional opinion, the data suggest that the cells of cross-bred multicellular organisms are actually smaller than the cells of inbred or pure line forms, and that the more rapid division is a function of the greater ratio surface has to volume in a small cell with the better opportunity thus obtained for increased metabolism.

That there is need of further investigation on size and variability in pure lines and in cross-bred forms through the application of statistical methods in connection with the maintenance of pedigrees through long series of generations seems evident. Eventually theories will make way for facts which will allow a proper perspective.

IV

Where do the results presented in the preceding pages lead us? Does their value, so far as their bearing upon the production of new and transmissible characters that will build up an organism in a required direction, consist merely in the formulating of hypothesis after hypothesis which as investigations proceed will in turn make way for other hypotheses equally transient? Or, on the other hand, do they mark a definite progress along the lines we are endeavoring to follow, namely, the control of evolution.

Before attempting a reply which must prove more or less unsatisfactory to those looking forward to immediate results, it seems advisable to pause for a moment and in the light of the preceding discussion con-

sider the types of differences—variations—which exist in so far as they may effect the result with which we are chiefly concerned.

Beginning at an early period in the history of evolution with the idea that all variations might be inherited, results soon suggested that the characters due solely to surrounding influences such as food supply, etc., were not thus transmitted. These were called *fluctuating variations.* On the other hand, variations due to the structural changes in the germ cells which were passed on from one generation to another have been spoken of as *inherited variations.*

The evidence at present indicates that farther subdivisions must be made and that normal inherited variations consist of two quite distinct classes. The variations where the results are due to the interaction of factors in accordance with Mendelian principle, and which, adapting a term used by Plate (1913), may be called *amphimutations* inasmuch as the condition is due to the mingling of two lines of descent, the other variations, as a class, in which the results —evolution in the abstract—are due to a series of units added as increments, may well be called *cumulations.* It is quite evident that the term "mutation" can not continue to include both types. As a coordinate term fluctuating variations may be spoken of as *fluctuations.*

Under abnormal variations must be classified forms ranging from monstrosities to slight departures from the ordinary condition, some of which are undoubtedly due to the losses or modifications of unit characters through the action of extraordinary stimuli, while others may be due to abnormal and unequal distribution of chromosomes occurring at the time of their division. The *idiomutations* of Plate are here included.

The answer to the question as to the progress made in the application of evolution to the creation of new forms rests in the statement that the attack on the problem is becoming more concentrated. The selection of fluctuations has been tried and has failed. Efforts by means of amphimutations end in a maze of circles with no evident progress. *Idiomutations,* so far as one may judge from the evidence, present retrogression rather than advancement. It is by means of pure lines under normal conditions that one may search with advantage for cumulations, the units by which to build the new. There the evidence will be unobscured either by the pyrotechnics of Mendelian formulæ, or by the factitiousness of abnormal stimuli. Fluctuations will be present, but statistical methods will permit their evaluation. Should the measurement of the mean in the tenth or even the one hundredth generation present no advancement, failure is not necessarily implied. Nature has devoted fifty millions of years or more to her work. There should be no discouragement if a few paltry years of investigation fail in duplicating her methods.

It is with a feeling not unmixed with pessimism, however, that one views the conditions under which work of the character outlined must evidently go forward. Those engaged in teaching have with a few exceptions time for little more than an occasional investigation of limited scope, particularly in a field which requires continuous application. Governmental departments where it could best be taken to a successful issue have only too often been subservient to political policies which demand immediate results. An ounce of compiled compendium is—to them—worth more than a ton of painstaking investigations which makes an advance on a theory. Looking a few generations into the future is not their concern.[6] A remedy for such conditions clearly

[6] Exceptional work has been done by those more or less closely connected with certain State Agricultural Experiment Stations. The names of East and

lies in endowments either in connection with universities, or through the establishment of the specialized private institution.

That the problem of applied evolution will eventually be solved there can be no doubt. That it will occur in our generation may only be expressed as a hope.

L. B. Walton

Kenyon College,
November 15, 1913

THE MUTATION MYTH

It has long been recognized both on the botanical zoological sides, that sterility is a notable characteristic of species crosses or true hybrids. Where species are nearer to one another their resultant cross is naturally less sterile than when their affinity is more remote. In the case of plants it is usually particularly easy to trace even slight evidences of previous hybridization in the sterility and abortive character of some of the spores or pollen. In contrast to hybrids, genetically pure species are characterized by pollen grains or spores, as the case may be, which are all perfectly developed. I have satisfied myself by a very extensive study of the spores and pollen of liverworts, mosses, ferns (including numerous genera of all the important families, isosporous and heterosporous), lycopods, selaginellas, quillworts, lepidodendroids, equiseta, cycads, ginkgo, conifers (including numerous genera of all the tribes), gnetales (all the genera) and many dicotyledonous and monocotyledonous angiosperms, that in good species the spores or pollen is invariably perfect morphologically, that is fully formed and having normal protoplasmic contents. Known hybrids on the contrary are characterized by a greater or smaller number of abortive spores, which have little or no protoplasmic contents.

Hayes, of Connecticut, Pearl, of Maine, Emerson, of Nebraska, Dean Davenport, Rietz and Smith, of Illinois, are familiar to all interested in the application of the principles of evolution. One often conjectures, however, as to the extent to which some of the most valuable contributions are in reality "by-products" of investigations meeting the approval of the "Missouri" type of legislator.

This conclusion is by no means new but the wide range of facts examined in the present connection adds very materially to its strength. It has been further noted that so far as morphological conditions are concerned, plants of genetic purity, even when grown under extremely abnormal conditions, as exotics in greenhouses, etc., have perfect spores or pollen. For example a conifer or a cycad from Australia or Japan, grown in the hothouse and producing its pollen in the winter season, still shows the grains normally developed morphologically, whatever may be their physiological inefficiency.

The bearing of the facts indicated in the paragraph above is of great importance in relation to the mutation hypothesis of De Vries. This distinguished Dutch plant physiologist, a little over a decade ago, published a series of observations and generalizations under the title of "Die Mutationstheorie." His notable offering was the statement that material of a species of *Œnothera* or evening primrose, referred by him to Seringe's *Œnothera lamarckiana,* found growing spontaneously near Hilversum in Holland, was producing annually new species or as he preferred to call them, elementary species. In 1904 Professor De Vries was invited to lecture in the University of California on his sensational discoveries. The lectures were edited and published later by the director of the Desert Laboratory of the Carnegie Institution of Washington, with the title of "Species and Varieties, Their Origin by Mutation." Dr. MacDougal thus became both in fact and figuratively, the "*vox in deserto clamantis,*" the baptist of the gospel of mutation. His exploits with the syringe in the baptism and production of new species of plants by intra-ovarial injections appear further to render his claims in this direction beyond question. As secretary of the Botanical Society of America and by his repetition and elaboration of De Vries's cultures of *Œnothera,* he has done unquestionably more than any one else to diffuse the doctrine of mutation in North America. It has in fact become so widely accepted on our continent, that it has in many instances

been unfortunately introduced even into elementary courses of botanical or biological instruction. Europe has presented a marked contrast to America in this respect, for here the mutation hypothesis of De Vries has been coldly received. It seems clear however that the European attitude is more in accordance with the established principles of biology than our own.

The distinguished English geneticist Bateson was apparently among the first to point out that in all probability *Œnothera lamarckiana* was a hybrid, as shown by the partial sterility of its pollen. Dr. MacDougal countered by the statement that he had found abortive pollen in specimens of *O. biennis*, growing in the vicinity of the city of New York. This he apparently regarded as a sufficient reply to Professor Bateson's criticisms. More recently a number of American students of plant genetics have cast doubt on the genetical purity of De Vries's *Œnothera lamarckiana*. Among these may be mentioned Davis, East, Emerson and Gates. The attitude of the last investigator is not the less significant because he was in the first instance a convinced mutationist of the De Vriesian school. In the case of the mutation hypothesis, we find again what is apparently an all too common condition in the case of biological theories, a neglect of fundamentally important morphological evidence. There seems to be absolutely no doubt, on morphological grounds, that not only is *Œnothera lamarckiana* of hybrid origin; but that the Onagraceæ, to which it belongs, are somewhat generally contaminated by spontaneous hybridism.

I have delayed publication of my conclusions in regard to the genetical status of the Onagraceæ, in the hope that some other morphologist would lay bare the extremely insecure foundations upon which the imposing superstructure of the mutation hypothesis has been raised. About eight years ago cultures of *O. lamarckiana* and its mutants were started at the Harvard Botanic Garden from seeds supplied by Professor De Vries. *Œnothera grandiflora*, which was of special interest in connection with the mutation hypothesis, was likewise grown from seed supplied by Professor S. M. Tracey who made a special visit to Tensaw to obtain it for me. Shortly afterwards other work became more pressing and monopolized all available time. The cultures and seeds were accordingly turned over to Professor Bradley M. Davis, who was residing in Cambridge at that time. Dr. Davis has published a number of papers on hybridization work with *Œnothera* species and I have supplemented my original stock of preserved material from specimens kindly supplied by him.

Some illustration of pollen conditions in known hybrids other than Onagraceæ will first be considered. Many of our improved horticultural plants are known to have originated by hybridization, while others more anciently cultivated by our species are suspected of hybrid origin. Taking only the known hybrids as illustrations, such as tulips, irises, narcissus, lachenalia, freesia, etc., etc., we find that although many of the pollen grains are perfectly developed both as regards external form and internal contents, a greater or smaller proportion are small, shrivelled and nearly or quite devoid of protoplasm. In the case of genetically pure species of these genera the pollen grains on the contrary are all alike and perfect. In normal species, even when long in culture and under hothouse conditions the pollen has proved in all the cases examined in the present connection perfectly sound. This is notably the case for example with the common easter lily. The effect of hybridization is equally clear in the case of the Dicotyledons. Our pinks, calceolarias, nasturtiums, etc., are often of known hybrid origin and show clear evidence of such derivation in the condition of their pollen, which is more or less abortive. President Brainerd, of Middlebury College, has in recent years made some interesting observations on spontaneous hybrids of our native violets. He has found numerous forms of these, significantly described by certain systematists as new species, to be in reality hybrids, as shown by their characters clearly intermediate between recognized species and by their Mendelian segre-

gation in cultures. Professor Brainerd has been kind enough to supply some flowers of these interesting specimens for the present work and the pollen conditions are clearly those found in hybrids.

We may now profitably turn our attention to the Onagraceae, to which the well-advertised *Œnothera lamarckiana* belongs. Our common garden fuchsias are known to be of hybrid origin. In some of the varieties the pollen sterility is almost complete. This is notably the case in a hybrid derivative of *Fuchsia triphylla,* grown in the Harvard garden. In other varieties the sterility is often less marked. This condition has been found to obtain for example in long-tubed fuchsia hybrids. Here a certain number of the grains are perfectly developed and have normal protoplasmic contents, while others are small, shrivelled, collapsed and without protoplasmic contents. Among the wild-growing species of the Onagraceæ, those of *Epilobium* have long been recognized by European systematists as prone to produce spontaneous hybrids. A good illustration in the present connection is supplied by a hybrid form of *E. hirsutum,* occurring commonly in the vicinity of ballast heaps. Here the pollen is to a large extent abortive, the degeneracy sometimes affecting the whole of the contents of the anther sack or in other cases being confined to a greater or smaller number of the grains. In contrast to *E. hirsutum* may be mentioned our common fireweed, *E. angustifolium* (sometimes put under a different genus). In all the abundant material of this species examined the pollen was entirely normal. Indications of hybridization correlated with corresponding pollen conditions have also been noted in the case of other representatives of the Onagraceæ, but the illustrations mentioned will suffice for our present purposes.

We may now turn to the genus *Œnothera* itself. In his "Mutationstheorie" De Vries has noted that about one third of the pollen of *O. lamarckiana* is sterile, and abortive. This statement I can only confirm. Even in the more vigorous of the so-called mutants originating in cultures of *O. lamarckiana* from the seed, the pollen is very largely degenerate and in the less vigorous elementary species often almost completely so. In *O. lata* (mutant of *O. Lamarckiana*) the pollen is frequently entirely sterile. But it is not only in *O. lamarckiana* and its so-called mutants or elementary species, that pollen sterility is to be seen, for this condition is well nigh universal in the species of this Onagraceous genus, recognized in systematic works. For example in the very common and variable species known as *O. biennis,* half of the pollen grains are sometimes abortive. This condition I have observed in specimens from regions as far apart geographically as the state of Massachusetts, the province of Ontario and the shores of the Gulf of St. Lawrence. The examination of a large amount of material of recognized wild species of *Œnothera* has led me to the apparently inevitable conclusion that spontaneous hybridism is extremely common in the genus and that in general it represents a condition of high genetical impurity. The purest species which has come under my notice, so far as may be judged from the pollen conditions is *O. grandiflora,* obtained from Tensaw. Here pollen abortion is well nigh absent both in flowers gathered for me by Professor Tracey and in those produced from seed both by Professor Davis and myself. Continued growth in the Harvard botanic garden has not altered this characteristic in any degree, for specimens grown last summer show the same condition of relative genetical purity or at least freedom from inharmonious hybridization.

It may be argued by some that the more or less marked constancy of the generally accepted species of *Œnothera* makes it clear that they are normal species. It is now recognized however that constant hybrids are of extremely common occurrence both in nature and as a result of experimental crossing. This is particularly true of species crosses. There is consequently no good reason why we should not admit that the genus *Œnothera* is strikingly characterized by spontaneous hybridization. There appears in fact to be every reason to believe that the bar sinister has been crossed and double crossed in our American

evening primroses. The extensive studies which I have made on the pollen and spore conditions in the higher plants from the mosses upwards, which will be detailed and illustrated elsewhere, make the conclusion apparently unavoidable, that the Onagraceæ in general and the genus *Œnothera* in particular, are peculiarly subject to spontaneous hybridization in nature. It follows of course that no genus or group of plants could have been more unfortunately chosen to illustrate the origin of species by mutation or saltatory evolution. Obviously we must in the light of the considerations advanced above, interpret the variability of the seedlings of *Œnothera* species, particularly of those of *O. lamarckiana* of De Vries, as evidence of ancestral hybridization, on the evidence of the very significant pollen conditions revealed both by the genus under discussion and by many members of the family to which it belongs.

The mutation theory of De Vries appears accordingly to lag useless on the biological stage and may apparently be now relegated to the limbo of discarded hypotheses. The zeal, industry and insight of the distinguished plant physiologist of Amsterdam can not be too highly appreciated. Even although his hypothesis must apparently be given up both on morphological and genetical grounds, it has nevertheless been the cause of a great deal of valuable work, which will remain after the motive of it has disappeared. The present refutation has been undertaken in the interest of biological progress in this country. It is now high time, so far as the so-called mutation hypothesis, based on the conduct of the evening primrose in cultures, is concerned, that the younger generation of biologists should take heed lest the primrose path of dalliance lead them imperceptibly into the primrose path to the everlasting bonfire.

EDWARD C. JEFFREY

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DEMOCRACY IN UNIVERSITY ADMINISTRATION[1]

A DISCUSSION of the topic assigned to me in this conference might be as brief as the famous chapter on snakes in the Natural History of Iceland, or, to use a more modern instance, as a review of Mr. Taft's activities in subversion of the courts and the constitution. There is no democracy in university administration. But we can consider the conditions and the remedies.

The situation of a teacher has aspects inherently undemocratic. He has arbitrary authority over the conduct and intellectual life of his students, and is paid by superior officials to discipline and teach as they prescribe. The professor may lecture to his classes "als dictirt euch der heilig' Geist," and in other academic relations may realize that silence is silver and flattery gold. To be half tyrant and half slave does not strike the average of a free man. The pedagogue may be expert in his narrow field, while he is segregated from the larger life of his fellow men. His salary is safe and small; his clothes are black and threadbare; he is very respectable, but only half respected. The inevitable difficulties we emphasize by providing on the one side a system of education which does not carry its own appeal and must be enforced by examinations, grades, degrees, compulsory attendance and the like, while on the other side a system of administration has developed which puts the professor in a position of personal dependence. He is not only unfree in the sense of the domestic servant, whose wages, work, company, habits and Saturdays off are set by the employer, but he is also unfree in the sense of the slave in that he is held to his place by forces that he can not resist. This may be in part caricature, like the typical professor of the novel or play who hunts beetles, while his daughter or wife engages in flirtation, but a caricature may depict and enforce the truth.

A less obvious but equally undemocratic aspect of the academic career is due to the fact that the university professor earns his living by teaching and the conduct of academic routine, while society depends on him

[1] Read at the conference on "The Relation of Higher Education to the Social Order," arranged by the council of the Religious Education Association, Yale University, March 5, 1914.

for productive scholarship and scientific research. Three quarters of the scholars and men of science in this country hold academic positions. Services to individuals can be paid for by those benefited, but we have no machinery in a democracy by which services to society are paid for by society. Public service can thus be rendered only by those who can spare the time, and is rewarded by recognition, reputation, honors, etc. Under aristocratic institutions men of inherited wealth may serve without salaries as members of parliament, magistrates, university chancellors, scientific men, scholars and the like, and may have their reward in social recognition, titles, membership in exclusive societies and similar non-rational sanctions. These by-products of oligarchy are its historical justification; responsibility for public service is placed on those who have wealth and privilege. But in a democracy power and wealth, in so far as they are desirable, should be the rewards of public service, not its prerequisites. Trustees of universities and members of school boards who serve without salaries are likely to render services about equal in value to the payment they receive.

Amateur work, whether by the man of wealth or by the teacher, becomes increasingly ineffective as the boundaries of knowledge are enlarged. The university instructor, impelled by sheer love, carries on a research, getting the time by working between hours and after hours. But he can not complete it or put it in its place in the orderly development of the science. He hopes to do so in the summer, but family bills accumulate, and he must engage in the sweat-shop labor of the summer school or some hack work. The research becomes cold, perhaps something of the same sort is done elsewhere, it is published in a slovenly way or not at all. I have somewhat recently had the privilege of visiting the Bureau of Standards, the Rockefeller Institute for Medical Research and the Research Laboratories of the General Electric Company. Here we have three institutions, conducted, respectively, by the government, under private endowment, and by an industrial concern, beside which the laboratories of physics, chemistry and physiology in our best universities are distinctly amateurish and inferior.

The men in these institutions have larger salaries and better facilities for their researches than are given in the universities; but their great advantage is that they are investigators by profession paid directly for the work they do. The professor, paid for his most important work in the fiat currency of reputation and petty honors, is in a position completely undemocratic. It is no wonder that we have the *demitasse* storms of academic politics and social life. There is one thing more absurd than for professors to march in processions in the order of their dignity advertising by brightly colored gowns and hoods the degrees they have received, and that is to make the financial reward of scientific and scholarly work transfer to an executive position which prevents doing such work thereafter.

The undemocratic aspects of our academic life are almost wantonly enhanced by the position attained by the president with the ensuing hierarchy of deans, heads of departments and other officials. The extraordinary material development of the country, with no balanced aristocratic system, has led to excessive power in the hands of a few individuals, whether in politics, in business, or in educational work. Every sensible person believes in individual initiative and individual responsibility. The safety in a multitude of counselors is usually due to the one who does the job. Government is a rough business, and this holds to a certain extent for educational institutions. The university or college president must do the best he can under hard conditions, and it is no wonder that he takes as much power as he can get. He has at least six masters—the trustees, the faculty, the students, the alumni, the general public and the bearers of the purse—not to speak of his wife's social and his own political ambitions. Each of them has different and discordant interests and ideals. It is not surprising that he finds it troublesome to ride these various horses and sometimes

. . . moves in a mysterious way
His wonders to perform.

The difficulty is that if an autocrat obtains unlimited powers, whether in the nation, the state, the city, the family or the university, he does not always prove to be wise and benevolent, and it may even be argued plausibly that the wise and benevolent despot is the worst kind, for he works the greatest demoralization. It is true that in a democracy we can afford to give large power to our leaders, for they are subject to the popular will. In the British democracy the monarch can only be permitted to be a social ornament, as he is there for life and his son after him. In our American democracy the president of the nation has extraordinary influence, but he can only maintain it so long as he reflects public sentiment. In Great Britain the cabinet is directly responsible to the parliament, and represents in its constitution the diverse elements of the majority, the prime minister not being necessarily the one most influential. This method is more democratic than ours, and in my opinion preferable. We have tried it with tolerable success in the commission form of government adopted by a number of cities. This is also at the present time being used in several colleges and universities, but not much can be expected here so long as it is a temporary expedient to last only until a president can be found.

It may indeed be seriously questioned whether the superior initiative and efficiency which one-man power is supposed to have is not more than counterbalanced in a university by the loss of these traits in the subordinates. A superman requires as his correlative many undermen. It is almost impossible to supervise the teaching and research of professors. Such an attempt is charmingly portrayed by President Maclaurin of the Massachusetts Institute of Technology in connection with the report on academic efficiency of the Carnegie Foundation:

The superintendent of buildings and grounds, or other competent authority, calls upon Mr. Newton:

Superintendent: Your theory of gravitation is hanging fire unduly. The director insists upon a finished report, filed in his office by 9 A.M. Monday next; summarized on one page; typewritten and the main points underlined. Also a careful estimate of the cost of the research per student-hour.

Newton: But there is one difficulty that has been puzzling me for fourteen years, and I am not quite. . . .

Superintendent (with snap and vigor): Guess you had better overcome that difficulty by Monday morning or quit.

The sinister side of the president's control of the professor is shown in two cases which have recently become public property. At Wesleyan University the professor of political science and sociology was compelled to resign after some remarks on the observance of the Sabbath and, at Lafayette College, the professor of philosophy and psychology was dismissed because his teaching was thought not to be in accord with the stricter standards of the Presbyterian church. We are not here concerned with questions of academic freedom or of permanance of tenure, but only with the methods of determining what the professor may say and how he shall be dismissed. As a matter of fact, in these two cases the alleged infractions of orthodoxy were slight. Several clergymen have told me that they might very well have made the remarks of the Wesleyan professor, and the Lafayette professor remains a Presbyterian clergyman in good standing. At Wesleyan, the president asked for an explanation of the remarks of the professor, demanded his resignation and accepted it, the three letters being written on the same day without the possibility of official consultation with the faculty or trustees. The fact that in this case the alleged ground for the dismissal was not the real cause does not improve the situation. At Lafayette, in like manner, the president wrote to the professor demanding his resignation in view of the supposed contents of a course. In this instance the professor was given a hearing before the trustees, but the president was naturally upheld.

A distinguished army engineer has recently stated that he would not accept the commissionership of police for New York City unless

the law should be changed so that a policeman might be dismissed without the right of appeal to the courts. It is supposed to be a part of the moral etiquette of the New York police to commit perjury in defense of one another, and it may or may not be that arbitrary power would for a time be desirable. But an army officer has no such control over his subordinates, who can only be court-martialed after definite charges and trial. One result of the difference between the police system and the academic situation is that no one can question the personal courage of the police. Whether it is better to lie like a policeman or to run to cover like a rabbit need not be argued, as it would doubtless be agreed that conditions should be such that this is not the necessary alternative. The slur about the third sex in America is unwarranted, but it would be better if there were not enough smoke to give rise to the alarm of fire.

Professors in the better institutions are not often dismissed because they or their views are not in favor with the administration, though this happens much more frequently than it becomes known, for the professor is naturally disinclined to drag the "pageant of his bleeding heart" across the continent and have his name put on the employer's black list. But it is this publicity which is his safeguard; and we have exercised by the body of professors and the general public a real democratic control, to which the president and trustees must submit. Stanford University has not recovered in thirteen years, and will not recover in another generation, from the loss of prestige due to the dismissal of Professor Ross and its sequelæ. Departments of economics and sociology in leading universities would not recommend a successor to Professor Fischer at Wesleyan, and public spirited men would not accept the position. At Lafayette, the resignation of the president has followed promptly the publication of the report of the American Philosophical and Psychological Associations on the dismissal of Professor Mecklin. On the other hand, Harvard maintained its high position by promptly offering lectureships to Professor Ross and Professor Fischer.

But while professors are not often dismissed because the president does not like their teaching or their personality, the possibility is present every day with a resulting demoralization not easy to estimate. Even more serious is the fact that the president may be responsible for the appointment and promotion of instructors and professors, and for increases in salary—for salaries are sometimes increased, however remote this contingency may seem to most professors. Semi-secret increases in salary by favor of the president must be regarded as intolerable. It tends to divide those who suffer under it into three classes—courtiers, quietists and rebels. The courtiers are those most likely to flourish in the system to its ultimate collapse.

I have had the privilege of proposing and seeing adopted by the trustees of Columbia University a change in the statutes in the direction of social democracy. We had long had, like some other institutions, provision for a sabbatical leave of absence on half salary. But in practise it proved that the sabbatical year was usually claimed only by those professors who had independent means or no family; it was thus a case of class privilege. Five years ago the statutes were altered to allow the alternative of a half-year leave of absence on full salary. This gives the professor some eight months for travel and research without loss of salary, and the institution sacrifices no more than on the half-salary basis, except in so far as more professors benefit. The plan deserves adoption in other institutions, and may properly be mentioned in a paper concerned with democracy in the university. It also gives opportunity for the frivolous remark that it might be an advantage if the statutes of a university provided for leave of absence of the president so often as he liked on double salary.

We do not know whether the progress of civilization has in the main been due to great men who have directed it, or whether these are essentially by-products and epiphenomena of social and economic forces. It is, therefore,

no wonder if we can not decide categorically whether or not it is well to have in the university one leader whom the rest of us will follow. But it is probably undesirable, as it is certainly undemocratic, to have a boss who drives us. This is the fundamental difficulty in our present university organization. The president is responsible to the trustees who in the private corporations are responsible to no one. The deans and heads of departments are responsible to the president who names them, and their subordinates are responsible to them. This department-store system reverses the correct or, at all events, the democratic direction of responsibility. The department or group should name its head and those to be added to it. The teachers or professors should name their deans and their president who should be responsible to them. The trustees should be trustees, not regents or directors. Their relations should be with representatives of the faculties, not exclusively with a president whom they appoint and who in practise is likely to select them.

It may be that the high-tide of presidential autocracy in our universities is now ebbing. At any rate we are discussing the problem more freely than in the past. I have obtained and published opinions of some three hundred professors who have done scientific work of distinction. These exhibit a very wide-spread dissatisfaction with the existing system. There is naturally much difference of opinion as to the remedies, but five sixths of them favor reforms in the direction of greater faculty control and less presidential autocracy. The remaining one sixth are mostly executive officers or men in institutions where the faculties have more than average influence. Thus the great university now entertaining this conference has maintained the better traditions. It has been said that if the faculties name the professors, there will be inbreeding and deterioration. To this it may be replied that Yale is represented in the National Academy of Sciences by eleven members; Cornell and Pennsylvania, with twice as many students, each by one member.

Harvard, like Yale, has maintained a measure of faculty and alumni control. President Eliot, whose masterful personality has been influential in exalting the presidential office, has at home deferred more to the corporation and overseers on the one side and the faculty on the other than lesser presidents. The plan adopted at Harvard of promotion after a fixed term of service with uniform increments of salary and permanence of tenure for the full professor removes him from the most humiliating relation to the president. At Cornell the faculties have been granted the right to elect their deans, and President Schurman advocates faculty representation on the board of trustees. At Princeton the departments have been authorized to recommend appointments and promotions, and a committee elected by the faculties meets with a committee of the trustees, this latter plan being in my opinion the most feasible method of improving the academic situation. Other reforms at various institutions in the direction of greater faculty control might be cited, the most striking and recent being the referendum vote of confidence obtained from the faculties by the president of the University of Illinois.

Whoever or whatever may be the occasion of reforms in academic control, the real cause must be the sentiment of the professors, and this can only be developed and expressed by proper organization. I am proud to belong to an association that at two consecutive meetings has taken action exhibiting a group consciousness of this kind. A year ago the American Psychological Association unanimously passed a resolution proposed by me to the effect that it is undesirable for its members to accept work in summer-schools or extension courses in which the pro rata payment is less than their regular salaries. Last Christmas at New Haven the association took the action to which reference has been made on the dismissal of the professor of philosophy and psychology from Lafayette College. An influential committee of one hundred on research has been formed by the American Association for the Advancement of Science. It may be that the time has now come when an association of American university professors might be or-

ganized, similar to the medical and bar associations, which would be an influential force in improving the conditions under which our work is done. It should not be forgotten that the maintenance of high standards in the university is as important for the community as for the professor, and his efforts on its behalf are by no means narrowly selfish. The future of the American university does not depend upon its machinery, but upon its men. The danger of a bad system is that it may gradually demoralize the spirit and ideals of the men working under it, and may keep from it or drive from it the kind of men who are needed.

When a speaker has only twenty minutes in which "to set the crooked straight," he can not be expected to devote much time to explaining that it is not so very crooked and is made of sound timber. The university is the noblest monument which we have inherited from the past and at the same time the most powerful engine driving forward our civilization. We owe to it the tribute of truth and the duty of service. It is our part to make it a democracy of scholars serving the larger democracy to which it belongs.

J. McKeen Cattell

CALVIN MILTON WOODWARD

Calvin Milton Woodward was born in Fitchburg, Mass., on August 25, 1837. He was graduated from Harvard in 1860 with the degree of A.B. and with the honor of membership in Phi Beta Kappa. In 1905 Washington University, and in 1908 the University of Wisconsin, conferred upon him the degree of LL.D.

During 1860–65 he was principal of the Newburyport, Mass., high school. In 1862 he was granted leave of absence for one year. During this period he served first as lieutenant and then as captain of a company in the 48th Massachusetts Volunteers. His regiment helped patrol the Mississippi in Louisiana and was under fire in the siege and storming of Port Hudson.

In 1865 he came to St. Louis, where in the service of Washington University and of his adopted city and state he passed the last forty-nine years of an active, energetic and fruitful career. At first he was the vice-principal of the academic department. In 1866 he was doing college work and was principal of the O'Fallon Polytechnic Institute. In 1868, under the authority of the university corporation, he began the organization of an engineering department. In 1870 he was made Thayer professor of mathematics and applied mechanics, and dean of the polytechnic faculty. In 1880 the St. Louis Manual Training School was opened, with Dean Woodward, its organizer, as director, and immediately it became the educational novelty of St. Louis, and for that matter, of America.

From this time, with some minor changes, he held until 1896 the positions of Thayer professor of mathematics and applied mechanics, dean of the engineering school and director of the manual training school. He resigned the deanship in 1896, but resumed the duties of that office in 1901 and again from that time carried his threefold official title until his final retirement from active service in the summer of 1910. He had remained in the harness until the close of his seventy-third year, when he retired upon the Carnegie Foundation. "His eye was not dim and" apparently, "his natural force was not abated." Four more happy years came to him in literary work, on educational boards and in the free use of his time and talent in the lecture field. On January 10, just passed, he was actively at work in behalf of a philanthropic enterprise which had deeply interested him for two or three years when the cerebral lesion attacked him which on January 6 proved fatal. After a private funeral service at the house, January 12, there was held at the church of which Dr. Woodward was an active member a memorial service at which Dr. Dodson, his pastor, Mr. Langsdorf, his pupil and colleague, as well as his successor as dean, Mr. W. A. Layman, president of the Wagner Electric Company, and Mr. Ben. Blewett, city superintendent of public instruction, spoke of his services to society, to the university, to mod-

ern scientific and civic progress and to general education.

Through all his busy years he lectured frequently in America on educational and scientific subjects and on special occasions in England. His contributions were numerous to periodicals and to the proceedings of the many learned societies of which he was a member. He was the author of two volumes upon manual training, one published by Heath & Co., the other in London and afterwards by the Scribners. During the years of his retirement he has written and published a notable text-book of more than 500 pages upon rational and applied mechanics. Every page shows the mature teacher and the clear logician reasoning with his students. Though published last year, the work is about to pass to a second edition.

It was fortunate for Mr. Eads, and for the engineering fraternity as a whole, that, from the bold inception to the triumphant completion of the St. Louis bridge, Dr. Woodward, then an enthusiastic young professor in his thirties, watched every operation and knew the great work to the minutest details. He went to the bottom of the piers, where men were working under more than four atmospheres of pressure. He was among the first to walk the plank connecting the approaching ends of the big middle arch. He knew the designs, drawings, contracts and unusual tests of material. With some of his students he was a passenger on one of the fourteen locomotives that tested the structure. When the work was done, which to this day stands in a class by itself, Captain Eads entrusted to Professor Woodward the task of writing the history of the achievement. This record occupied two busy years and remains an enduring monument to its author and to James B. Eads. At the time of its appearance it was said to be the most important contribution to engineering literature that had appeared in America. The book is widely distributed among the scientific libraries of the country; but the original edition is nearly exhausted and another will probably never be printed.

About that time, Professor Woodward, impelled by the serious needs and deficiencies of those of his students who were ambitious to become engineers, conceived and developed the manual-training idea. Upon this phase of his work it is not necessary for us to dwell. The innovation had many and bitter enemies, for it was not then apparent how manual training could help in making educated men. But a large section of the educational world in America now believes in it and has adopted it in many secondary schools as one element in the formation of the efficient citizen.

At a critical time in the development of the University of Missouri Dr. Woodward was a curator and president of the board. His service at that time to higher education in his state will never be forgotten. While the Louisiana Purchase Exposition was in progress at St. Louis, Dr. Woodward was president of the Aeronautical Congress which did much to create a strong faith throughout the country that the conquest of the air was near at hand, and he himself made important contributions towards the solution of the problem.

In 1894 Dr. Woodward was president of the Society for the Promotion of Engineering Education, and in 1909–10 he was president of the North Central Association of Colleges and Secondary Schools, the first time in the history of that useful organization that a college professor not of presidential rank presided over its deliberations.

Dr. Woodward was chosen president of the American Association for the Advancement of Science for the meeting of 1906. That the choice was a happy one no one could deny who was fortunate enough to be present at the opening session in New Orleans. Since the Civil War this was the first visit of the American Association to a southern city. Southern hosts and northern guests vied with each other to make the occasion a delightful one. As usual the first formal session of the meeting was given up to courteous welcomes and responses. The splendid way in which President Woodward rose to the possibilities of graceful speech none of those present will ever forget.

Dr. Woodward served his city conspicuously

in many matters pertaining to its growth and development. At one time the city census was questioned and he was chosen superintendent to repeat and verify it. He was always ready and forcible in the discussion of the engineering problems of the city, and served terms as president of both the St. Louis Engineers Club and of the St. Louis Academy of Science. As a member of the city school board and several times as its president, he has given character and direction to the public school system of St. Louis.

Professor Woodward married Fanny S. Balch, of Newburyport, Mass., in September, 1863. She survives him together with three daughters, two of whom are married. His home life was an ideal one; to enter within its circle was always a privilege.

In hasty review we have selected only a few of the instances where Calvin Milton Woodward has come prominently before the country. The story of a long life of cheerful labor and distinguished service in the college halls can never be told. There is no tangible record of the daily lessons enforced with an unfailing and overflowing spirit of optimism. But it is this work and this spirit that produce a sure and lasting effect upon the lives of students. Just as the gentle sunshine is the most potent force in nature, so the efficient teacher, who on the whole is quite often an object of condescending sympathy in our social state, is nevertheless the mightiest agent in the progress and development of society, for he is developing its coming leaders and, therefore, more than any other agency, is shaping its destiny. Happy is that teacher who knows his power and lives true to his high calling. His name may soon be forgotten, but the essence of his life and labor passes on from heart to heart and from generation to generation.

Such a man was Dr. Woodward. As a teacher he lived. As a teacher he sought his life's reward. A little more than a year ago in a company where were many of his former students he expressed his conviction that no epitaph could more highly honor him than the simple statement "He was a teacher of men."

C. A. Waldo
Washington University,
St. Louis

Robert Kennedy Duncan

By the death on February 18 of Dr. Robert Kennedy Duncan, director of the Mellon Institute of Industrial Research of the University of Pittsburgh, American science lost one of its most illustrious devotees. Dr. Duncan was known to the public at large in two important lines of service. First, as an interpreter of science, in which branch he was preeminent; he gave life to the most abstruse things of cold science and made them of intense interest. His books, "The New Knowledge," "The Chemistry of Commerce" and "Some Chemical Problems of To-day," while of the highest scientific accuracy, are so written as to hold the reader's sustained attention to the end. The other line in which he will be remembered is as the originator of the unique system of *the service of science to industry*. Dr. Duncan felt that he was fortunate in being able to live to see this system established on a permanent basis in the Mellon Institute of Industrial Research. With his usual farsightedness he carefully trained those who were to take his place when he was gone and the institute embodying the system he originated will go on as a living monument to its founder.

Certain of the ideas he had in mind in working out a practical method whereby the learning of the university could be brought to the service of industry are interesting, in that they show prominent traits of his character. He once told me that he experienced the keenest pleasure of achievement when he thought of the opportunities he was able to offer to young men through the working of this system. He loved to speak of his laboratory as a center of opportunity for young men. And that fact really indexes the keynote of his character. He was absolutely unselfish. He sought nothing for himself and was continually trying to advance his "boys,"

as he loved to call them. And he was in his relations to them an older brother more than a formal director. Dr. Duncan always felt that in scientific investigation the things of the spirit were more important than material things and that the many seemingly impossible results which had been achieved in his laboratories were accomplished solely because of the fine spirit which he instilled into his men.

Dr. Duncan was born in Brantford, Ontario, 1868. He graduated with first-class honors in chemistry and physics from the University of Toronto in 1892 and did graduate work in Clark and in Columbia universities. He was a preparatory school teacher of chemistry from 1893 to 1901, when he became professor of chemistry at Washington and Jefferson College (1901–6). He studied abroad during the years, 1900–1903, 1904 and 1907. Up until 1906, Professor's Duncan's life as it was evidenced to the outside world was simply that of a successful growing teacher of chemistry. But in his inner life a great purpose had been developing and finally took form in his system of industrial fellowships. He really led a dedicated life—a life dedicated to the working out and founding of a method of sympathetic cooperation between industry and learning, whereby industry, the university, the public and young men would be greatly advantaged. In 1906 Dr. Duncan was called to the University of Kansas as professor of industrial chemistry and it was then and at that time that he initiated on a small scale his new system of industrial fellowships. In 1910 he became director of the department of industrial research of the University of Pittsburgh, which department, in 1913, became the Mellon Institute. It was in the University of Pittsburgh that industrial fellowships expanded with such extraordinary rapidity to the present large institution.

As an educator in the field of science Dr. Duncan's influence was very far reaching. He had the rare faculty of imparting to his students and associates his own overflowing enthusiasm. He inspired a peculiar loyalty to the highest ideals of scientific truth. Numerous short articles from his pen, in addition to his books—written with a unique charm of expression and power of interpretation—gave a widespread interest in things scientific. His investigations and writings on the conditions of the employment of chemists by industrialists had a marked effect in bettering the status of the industrial chemist.

As to the personal influence of his life on those with whom he came in contact, I do not know how better I can express this than by appending the following tribute from one of his "boys."

RAYMOND F. BACON

ROBERT KENNEDY DUNCAN, AS ONE OF HIS BOYS KNEW HIM

THERE are scattered over this country a fairly large number of men each of whom is glad if Robert Kennedy Duncan ever referred to him as "one of my boys." Yes, there are many of us who are better men, who see the values of this life clearer for having been one of his "boys." To some of us he was a professor of chemistry, to some he was a director of industrial research but to all of us he was a big brother, yes even a father. He was an employer of none of us in the ordinary sense of the term. Robert Kennedy Duncan was truly "a fisher of men," a witness of the light which has ever shone clearly for those who have lived the gospel of man's brotherhood.

I wish I had the pen to enable others to understand something of his leadership. None can see all the facets of a gem from one angle. Neverthless I shall attempt to speak of him, as one of his "boys" who has been privileged to know him.

I can do no better than to select a few of his own sentences about certain deeds of men which excited his own admiration. He was fond of military analogies. In a recent article on industrial research which appeared in *Harper's Magazine* he likened the modern spirit of discovery in scientific research to the spirit of the young Marco Polo.

Let us contrast synthetic rubber with synthetic indigo. The commercial synthesis of indigo was

accomplished after a Kitchener-like advance, in which each step at a time was buttressed and battlemented by coordinated facts until the summit was attained and the fortress was won. It was an irresistible march of the horse, foot and artillery of scientific endeavor. The attack upon rubber, on the other hand, recalls nothing so much as the raid of the adventurers accompanying Cortes into the wilds of Mexico. Ludicrously few in number and ill equipped save with a *dauntless spirit*, they plunged desperately into a wilderness absolutely unknown and denizened by countless thousands of a malignant and disciplined enemy; yet they conquered Mexico. The conquest of Mexico was incredible, it was *unreasonable* to the military tactician; so is the conquest of rubber incredible to the tactician of scientific research.

Allow me to quote another sentence which I think shows one of the great elements of his leadership.

> Of course I have forgotten something. I have forgotten the afternoon tire in the garish light of the laboratories, the hard cot at night by the laboratory table, the broken experiments, and the heart-breaking disappointments to endeavor. But so did Marco Polo, for you will look in vain through all his glowing pages for the bitter cold of the morning camp, or the intolerable heat of the desert or of the pain of insect pests, or of his sorrow at the loss of his goods—all forgotten in the retrospect of his wonderful journey.

Valery-Radot wrote two delightful volumes giving us a picture of Louis Pasteur, yet there are two incidents which to me are most illuminating and recall Doctor Duncan. The author tells us of Pasteur's anguish on the occasion of the death of a boy from rabies. Pasteur forthwith undertook an investigation of the subject. Finally one sees this great, tender-hearted man with all the wonderful vision of his mature years brought near death's door and in one last spasm of effort crying: "We must work. We must work." And so Robert Kennedy Duncan was removed by the accident of death just as his dreams, his great constructive dreams, for the amelioration of man seemed to be in the dawn of their fulfillment. Only last December Doctor Duncan attended the Atlanta meeting of the American Association for the Advancement of Science and met Dr. Howard A. Kelly. Sixty thousand people dying of cancer in this country every year! He returned from that meeting burning with a desire to do something. The idea that radium is and undoubtedly always will be beyond the reach of thousands of these sufferers, *that* was what appealed to him. Couldn't something be done about it? He believed there could. He told his ideas to a Pittsburgh business man, who very quickly said, "It would give me real pleasure to help you tackle this thing."

Another sentence of Doctor Duncan's will always stick in my mind and I think gives us a clue to his fine spiritual nature. In discussing the synthesis of ammonia from nitrogen and hydrogen he recalls that up to 1908 the best work of Haber, Nernst and others had failed to give results of industrial promise and as Dr. Duncan said of Nernst's work:

> With this final investigation, then, it was "thumbs down" for the subject; it was finished, exhausted, dead.

But Professor Haber had a feeling that the thing could be done. Doctor Duncan says:

> It is to be understood that this "feeling" which possessed Haber was not the obsession of an ignorant dreamer but was actually the expression of a *faith that lay deeper than reason* on the part of one who knew, possibly better than any one else from the standpoint of reason, its folly.

The splendid qualities which he so admired in others he himself possessed. It has ever been such rare spirits which have done the impossible, have pointed the way.

THE WELLESLEY COLLEGE FIRE

The fire at Wellesley College on March 17, which totally destroyed College Hall, the oldest and largest buiding, has brought great loss to the college and has greatly disabled four science departments.

College Hall, which originally contained the whole college community, at the time of the fire was a dormitory for two hundred and fifteen students, and also held the offices of the administration, the lecture rooms for the greater part of the college, and the laboratories of the departments of geology, physics.

psychology and zoology. The entire equipment and the collections of each of these four sciences are destroyed, and the department libraries of geology, physics and zoology. As a consequence, these departments are seriously crippled and are in great need of assistance.

The collections of the geology department were very valuable and some were very rare mineral specimens. Recently many new cases had been acquired and space for exhibition. The lantern of this department was the only piece of apparatus that survived the fire, but the thousands of lantern slides were destroyed.

The more important losses to physics are lantern slides, collection of crystals, a unique collection of Nicol prisms, and complete files of the important scientific journals, some dating back to 1800.

Besides the actual equipment, the most serious loss to the psychology department is the destruction of its records of experiments, memory and intelligence tests on normal and abnormal subjects, the results of several years of work.

The zoology museum was far richer than was generally known. It was inadequately housed and crowded, and its specimens were never displayed to advantage. The collections represented the results of many years of labor and of careful selection, and were essentially study collections, planned for special courses, and constantly in use by different groups of students. The losses which will be felt most keenly by the individual courses are the North American birds and insects, the general invertebrate collections, recently enriched by material from the zoological station at Naples, the mounted and disarticulated skeletons, the histology and embryology slides, and the physiology apparatus.

The personal losses of the teaching staff are very great. In the zoology and psychology departments alone, original work, drawings, notes, collections, microscopes and apparatus, books and reprints, all are gone.

Aid has come already in generous measure to the four stricken departments from many colleges and museums near Boston, from Clark University, Mt. Holyoke College, the University of Pennsylvania, Vassar College, and from former students and friends of Wellesley; and material has been lent and given that will enable the scientific courses to reopen with the rest of the college on April 7, in the laboratories of the departments of astronomy, botany, chemistry and hygiene, all of which are in separate buildings and are therefore untouched by this disaster.

Our future needs are very great, buildings, equipment, material for work, museum specimens, books. May the realization of these needs bring yet more help to our support.

CAROLINE BURLING THOMPSON
WELLESLEY COLLEGE

THE PRESIDENCY OF THE UNIVERSITY OF IOWA

THE president of the University of Iowa has, under the date of March 20, 1914, addressed to the Iowa State Board of Education, the following letter:

By this letter I submit to you my resignation as president of the State University of Iowa, to take effect at your earliest convenience. Some explanation of this action is due to you and to those interested in the welfare of the university. Such explanation follows:

At the meeting of your board held at Cedar Falls, March 11, you considered in executive session a number of administrative matters concerning the university. Among other things at that time you dismissed a professor of the university without a hearing and without the knowledge or advice of the chief executive of the institution. Whether or not the facts, if you have them, warranted the professor's dismissal is not now the issue to which I call your attention; and I pass over for the moment the obvious fact that the professor himself had a right to be heard. I can not avoid the inference that your action is deliberately intended to express lack of confidence in the administration of the university.

Before I came to the university in 1911 I asked you in writing to consider thoroughly the step you proposed; it was for you to decide whether or not I was the man for the place and I called your attention to this fact. As part of the terms on which I finally accepted the position you agreed in writing that all recommendations for appoint-

ment and for dismissal in the university should come through my office to your board. This is the procedure in any well-governed university. At that time, also, I asked explicitly that if at any time you should feel that you would be more comfortable with another man in my position, you express to me frankly that feeling; and I stated that I would, then, with good will, promptly retire. To this also you agreed.

I can not but regret that when the time came to act under this agreement, you did not do so, frankly and honorably. You had only to ask for my resignation to receive it at once. The course you adopted to accomplish the same object is unworthy of yourselves, unjust to me, and involves a still more serious injustice to another man whose case should have been considered on its merits after proper hearing and investigation, and not entirely subordinated, as I believe it was, to the evident desire to raise, quite needlessly, a personal issue between your board and myself.

It is not my purpose to dwell on this point, however. Interpreting your action as I have no doubt it was intended, and conforming on my own part to the understanding on which I came here, I willingly withdraw from a position which I accepted only at your earnest solicitation and which I have no desire to retain unless I can demand and receive your entire support. I do this with the less regret because as I recall the issues which have arisen between us and which have led to your present attitude, I am as firmly as ever convinced that the ideals and policies which I have held for the university during the past two and one half years have been right and educationally sound; that no single instance of personal politics or self-seeking on my own part has contributed to our differences; and that if you had chosen to give me a reasonable opportunity for working out these ideals and policies they would have justified themselves in ample measure by the results.

If it were possible that I have exaggerated the significance of your action in its relation to myself, my course would still be the same. No man can be held accountable for his responsibility unless his authority is respected, or for his policies unless he is given a free hand to carry them out. This principle is recognized in all well-governed colleges and universities, as it is in every well-organized business. The efficient government of this university on any other basis is impossible, and I could not be a party to so hopeless an experiment even if it were your desire that I should.

SCIENTIFIC NOTES AND NEWS

At the annual meeting of the National Academy of Sciences, to be held in Washington on April 21, 22 and 23, the William Ellery Hale Lectures will be inaugurated by two lectures on "The Constitution of Matter and the Evolution of the Elements," by Sir Ernest Rutherford, of the University of Manchester.

A special convocation held at Oxford on March 24 conferred the degree of doctor of science on Surgeon General Gorgas.

The Fothergill gold medal of the Medical Society of London for 1914 has been awarded to Dr. John George Adami, F.R.S., LL.D., Strathcona professor of pathology and bacteriology at McGill University, for his work on pathology and its application to practical medicine and surgery.

We learn from *Nature* that the council of the University of Birmingham has appointed Professor Charles Lapworth emeritus professor of geology in recognition of his services during his occupation of the chair of geology. The senate recently signalized his retirement by the presentation of an address and a gift of plate, and on March 11 another presentation was made to him by a large number of his old students.

The last number of the *Münchener medicinische Wochenschrift* is a special issue in honor of the sixtieth birthday of Professor Ehrlich, which occurred on March 14.

Dr. Lawrence Martin, of the University of Wisconsin, has been elected a corresponding member of the International Committee on Glaciers.

Mr. R. J. Pocock, of Queen's College, Oxford, has been appointed to direct the observatory of the Nizam of Hyderabad.

Professor F. Keeble, F.R.S., professor of botany, University College, Reading, has been appointed director of the Royal Horticultural Society's garden at Wisley.

Dr. John W. Colbert, Albuquerque, has been asked by the Rockefeller Foundation to assume charge of its research work in a campaign to be inaugurated for the eradication

of hookworm disease in Central and South America.

MR. H. GLAUERT, of Trinity College, and Mr. H. Jeffreys, of St. John's College, have been elected to Isaac Newton studentships in the University of Cambridge.

THE Sarah Berliner Research Fellowship has been awarded for the year 1914–15 to Miss Ethel Nicholson Browne, Ph.D. (Columbia), now instructor in biology at Dana Hall, Wellesley, Mass. Miss Browne will spend the year at the University of Würzburg and at the Zoological Station at Naples, doing research work in cytology.

THE Kansas City Section of the American Chemical Society celebrated its one hundredth meeting on March 21. This section was chartered in 1900 and has been holding alternate meetings in Lawrence, Kansas, and in Kansas City, Missouri. At this meeting Dr. Wm. McPherson, of the Ohio State University, was the guest of honor and delivered an address illustrated with lantern slides upon "European Laboratories and Chemists."

FROM May 5 to June 23 a course of eight lectures on the rate of the blood-flow in man in health and disease will be given in the physiological laboratory of London University, by Dr. G. N. Stewart, professor of experimental medicine, Western Reserve University.

IT is stated in *Nature* that the Faraday Society arranged a general discussion on optical rotatory power, to be held in the afternoon and evening of Friday, March 27, in the rooms of the Chemical Society, Burlington House, Professor H. E. Armstrong and Professor Percy F. Frankland, presiding. Papers on various aspects of the subject were to be read by Professor Hans Rupe (Basle), Professor H. Grossmann (Berlin), Professor Leo Tschugaeff (St. Petersburg), Dr. Darmois (Paris), Dr. T. M. Lowry, Mr. T. W. Dickson, Mr. H. H. Abram, Dr. R. H. Pickard, Mr. J. Kenyon and Dr. T. S. Patterson.

DR. HARRY BURROWS, lecturer in chemistry in the Sir John Cass Technical Institute, London, died on March 15, at the age of forty-two years.

PROFESSOR JOACHIMSTHAL, director of the University Hospital at Berlin for the surgical treatment of cripples, died on February 28, aged fifty-two years.

DR. ADELBERT VON WALDENHOFEN, emeritus professor of applied physics in the Vienna Technological School, has died at the age of eighty-six years.

THE House committee has favorably reported the bill providing that the historic botanic garden, located for many years at the foot of Capitol Hill, be removed to Rock Creek park, in the far northwest section of the city. The bill also provides that the garden is to pass from the direct control of congress to that of the department of agriculture.

MR. JOHN LAMBERT CADWALADER left $195,000 to public institutions with which he was associated, and gave valuable works of art to the Metropolitan Museum, and books to the New York Public Library. The public institutions which received legacies are the New York Public Library, $100,000; the Metropolitan Museum of Art and Princeton University, $25,000 each; Harvard University Law School and the New York Zoological Society, $20,000 each, and the Alumni Association of the Harvard Law School, $5,000.

A MATHEMATICS CLUB is in process of organization at the Ohio State University. Professors and students will have an equal share in discussing mathematical literature and some of the newer developments in the science. At the first meeting of the club, Professor R. D. Bohannan, head of the department of mathematics, will speak on the spirit of the old mathematics as related to the new, showing the quality of research, and of critical investigation which has taken the place of former unthinking respect for authority.

THROUGH the generosity of M. Spendiaroff of St. Petersburg, the International Geological Congress presents at each session a prize amounting to about 450 roubles for the best

work in some specified field of geology. The next prize will be awarded at the session in Belgium in 1917 for the best work in petrography giving new light on the general problems of the science. Two copies, at least, of any work presented for the competition must be sent to the general secretary of the last congress, R. W. Brock, deputy minister of mines, Ottawa, Canada, at least one year before the next session.

The Christiania correspondent of the London *Times* states that according to interviews in the Norwegian papers, it looks doubtful whether Captain Amundsen's expedition can start in 1914. The *Fram* appears to be delayed and must be at San Francisco at latest at the beginning of July. If delayed Captain Amundsen will use the time in order to practise his aviators and scientific staff. The *Fram* will take three aeroplanes. The German Antarctic explorer Captain Filchner has been engaged to act as observer and photographer. Dr. Nansen will next summer undertake an oceanographic expedition with the Azores as a central point.

A joint meeting of the Association of American Geographers and the American Geographical Society is being held on Friday and Saturday, April 3 and 4. The program is as follows:

THE EVENING LECTURE (ENGINEER'S HALL)

L. A. Bauer: "The General Magnetic Survey of the Earth."

FRIDAY MORNING SESSION (AMERICAN GEOGRAPHICAL SOCIETY'S BUILDING)

W. H. Hobbs: "Land Sculpturing in Arid Lands with Observations from Northeastern Africa."

FRIDAY AFTERNOON SESSION (AMERICAN GEOGRAPHICAL SOCIETY'S BUILDING)

T. Wayland Vaughan: "The Platforms of Barrier Coral Reefs."

D. W. Johnson: "Botanical Phenomena and the Problem of Coastal Subsidence."

E. W. Shaw: "Characteristics of the Mississippi Delta in the Light of Comparative Studies of Some Old-World Deltas."

Oliver L. Fassig: "The Period of Safe Plant Growth in Maryland and Delaware."

SATURDAY MORNING SESSION (AMERICAN GEOGRAPHICAL BUILDING)

Frederick J. Turner: "Geographic Influences in American Political History."

J. Russell Smith: "The Tree as a Factor in Man's Adjustment to Hilly and Rocky Land."

W. W. Atwood: "Over the San Juan Mountains to the Ancient Cliff Dwellings of the Mesa Verde."

Collier Cobb: "The Forest of Sunburst: A Study in Anthropo-geography."

Spruce, abundant in the New England and Lake States and in Canada, has heretofore been the standard wood for making news print paper and as long as there was a supply sufficient to meet the needs of the paper industry there was no reason to seek substitutes. But heavy inroads have been made on the spruce forests of the western part of the United States in this day of great circulations and large editions, especially of Sunday papers with their many parts. On a rough estimate, a newspaper with an average circulation of sixty thousand copies and an average edition of twenty pages, uses each day the product of about four acres of forest. When this figure is multiplied by the great number of newspapers published in the United States, many of them with much larger editions, and when this is further multiplied by 365, because many papers are issued every day of the year, it can be seen that the drain upon the forests is enormous. Foresters say that even under the most approved methods known to their profession, it could scarcely be expected that spruce would be able to hold its own, but would need supplementing by other material. It is but natural, therefore, that paper manufacturers are looking for new sources of supply which will furnish an abundance of wood pulp, at a price which will not be prohibitive. Poplar and a few other woods are used, but they do not go very far. In the national forests there are many woods considered inferior by lumbermen. Yet they are available for purchase at low rates and many of the timber stands are readily accessible. The forest service, in its desire to utilize to the best advantage all of the resources of the federal timber holdings,

has been seeking proper uses for these trees and has experimented in making pulp from them at its pulp laboratory at Wausau, Wisconsin, an auxiliary of the forest products laboratory at Madison. The Wausau laboratory is equipped with standard machinery and all experiments are carried out under conditions which duplicate commercial practise. As a final test of the value of some of these new woods under practical conditions, arrangements were made between the forest service and the *Herald* to print some part of its edition on paper made from various woods that showed promise as substitutes for spruce. These woods were ground at the Wausau laboratory; the product was then mixed with the usual proportion of chemical pulp and made into news print paper, rolls of which were sent to New York for the experimental run.

SETTLERS in western Kansas are cutting and marketing soap weed, or Spanish bayonet, to supply the demands of soap manufacturers, according to a report recently received from officers of the Kansas national forest. There are various plants in the southwest locally known as soap weed, called amole by the Mexicans, but the one gathered by the Kansas farmers, technically known as *Yucca bacata*, a species with exceptionally large fruits, is the most used. The soap manufacturers, however, utilize the tops or the roots. Manufacturers are paying $8 a ton for the plant at the railway stations, while the estimated cost of cutting, drying, baling and hauling ranges from $5 to $6, depending upon the distance to the railroad. Since a man can ordinarily get out a ton a day, the gathering of the soap weed affords an opportunity to secure a fair day's wages at a time when other ranch activities are not presssing. After cutting, the soap weed is allowed to dry from 60 to 90 days and then is baled up in the ordinary broom-corn baling machine. For a long time this weed has been made into a soapy decoction which the Indian and Mexican women have used, particularly for washing their hair, for which purpose it is considered especially suited, since it contains no alkali. Present-day soap manufacturers use it for toilet and wool soaps. Its qualities have been known for a long time, but the harvesting of soap weed is just now becoming commercially important. The industry is now operating on lands adjacent to the Kansas national forest and it is expected that the demand will soon spread to that forest, some portions of which bear an abundant supply of the plant. There is a plentiful supply of it throughout southern Colorado, Arizona, New Mexico and Texas. Forest officers have considered this weed a nuisance, since it is the nature of the plant to spread over extensive areas and kill off other vegetation. It is particularly a pest on stock ranges. In line with its policy of range improvement, the government is anxious to rid the forage areas of all such injurious plants, and it is the hope of the forest officers that the commercial demand for soap weed will soon reach such proportions that it will not only take an otherwise useless product, but also will eradicate it from areas which could be utilized to better advantage for the supplying of forage to cattle and sheep.

IT is generally recognized that boric acid in considerable quantities is an original constituent in the waters and gases given off with volcanic emanations. In fact, the Tuscan fumaroles, in Italy, have been an important commercial source of boric acid for a long time, and in the past, possibly even to the present time, almost all the boric acid brought into the European market has been derived from this source. There is abundant evidence of the presence of boric acid in volcanic emanations in many parts of the world. On the other hand, boron is so rare a constituent of rock-forming minerals that it forms an almost inappreciably small percentage of the earth's rock mass as a whole. A short study of the borate deposits in Ventura County, Cal., supplemented by more cursory examinations of similar deposits in the vicinity of Death Valley, has been made by Hoyt S. Gale, of the United States Geological Survey, and a new theory of the origin of the deposits of colemanite, or borate of lime, in these regions has been advanced by Mr. Gale in Professional Paper 85, Part A, recently published by the Survey. While this theory has not yet been entirely proved, there is much in its favor and it affords suggestions and a working basis

for further observation. The supposition of a desiccated saline lake to explain the origin of the colemanite has little to support it beyond rather general assumptions. The character of the deposits themselves indicates rather a vein type of formation. Other salines which would naturally be expected in desiccation deposits resulting from natural saline solutions are not found in association with the colemanite. Those who have supported the desiccation theory have offered no explanation of the cause which might produce colemanite in such massive deposits as a product of water evaporation, while, on the contrary, its formation from limestone in veins by replacement of carbonic acid with boric acid is a natural hypothesis that deserves further investigation. The relations of the deposits to basalt lava flows indicate the probable origin of the boric acid at the time of the extrusion of these lavas, although it may be assumed that this acid continued to find its way into solution of the circulating ground waters long after the period of the extrusions.

UNIVERSITY AND EDUCATIONAL NEWS

Provisions for the creation of a trust fund said to be approximately $500,000 for the maintenance of male graduates of the Williamsport, Pa., high school at Cornell University are made by the will of Albert Dubois Hermance.

Mr. Edgar Palmer, a Princeton graduate of the class of 1903, has offered to build and present to Princeton University a stadium costing $300,000. Mr. Palmer is a son of the late Stephen S. Palmer, who was for many years a trustee of Princeton University and gave large sums to the university, including the Palmer Physical Laboratory.

Through the cooperation of the estate of the late Dr. C. Annette Buckel, of Oakland, a research fellowship for the study of feeble-minded children has been established at Stanford University. The department of education, under the direction of its head, Professor E. P. Cubberly, will have the appointment of the fellow, who will work in cooperation with Professor Lewis M. Terman. Dr. Buckel was an Oakland physician known for her charitable work in Oakland and for her interest in feeble-minded, backward and delinquent children. On her death her estate was left in trust to Miss Charlotte S. Playter, of Piedmont, to be used to advance the condition of backward and feeble-minded children. Miss Playter has turned the money over to Stanford. The income amounts to about $500 a year, and the board of trustees of the university have added an additional $500 to the fellowship.

Work has begun on a temporary recitation and administration building for Wellesley College. It will be a wooden structure, one story high, of the simplest possible construction. It is contracted to be finished by May 1. Classes will begin on April 7, the regular date for the opening of the spring term. There are no plans as yet to replace the geological, physical, psychological and zoological laboratories which were destroyed by the burning of College Hall.

The recent disastrous fire at Wellesley College wiped out the entire equipment of the department of physics. This department, organized in 1878, was one of the first in the country to offer laboratory practise for undergraduates and possessed much apparatus of value. Within the last few years extensive additions had rendered the equipment thoroughly modern and up to date. The library of nearly three thousand volumes contained complete files of most of the leading periodicals, English, French and German, including the *Annalen der Physik*, the *Philosophical Magazine*, and the *Philosophical Transactions* since 1800. The loss is total.

Yale University and the University of California will exchange professors next year. Professor John Wurts, of the Yale Law School, will lecture at California, and Professor G. H. Boke, of the School of Jurisprudence at California, will lecture at Yale.

Dr. Thomas H. Macbride, professor of botany, has been appointed acting president of the State University of Iowa.

Dr. Nathaniel E. Loomis, assistant professor of chemistry at Bowdoin College, has

accepted a professorship of physical chemistry at Purdue University.

DR. L. D. BRISTOL, now of Syracuse Medical School, has been appointed to succeed Dr. G. F. Ruediger as director of the public health laboratory of the University of North Dakota. Dr. R. T. Young has been appointed professor of zoology and succeeds Dean M. A. Brannon as director of the University Biological station at Devil's Lake.

DR. PRAFULLA CHANDRA RÂY has been appointed to the Sir Taraknath Palit professorship of chemistry, and Mr. C. V. Raman to the Sir Taraknath Palit professorship of physics in the Presidency College, Calcutta.

DISCUSSION AND CORRESPONDENCE

DADOURIAN'S ANALYTICAL MECHANICS AND THE PRINCIPLES OF DYNAMICS

PROFESSOR E. W. RETTGER'S review of my "Analytical Mechanics," which appeared in number 995 issue of SCIENCE, gives a wrong impression of my treatment of the principles of dynamics.

The reviewer's criticisms are directed, mainly, against my claim of having based the science of mechanics upon a single dynamical principle. Starting from certain premises, which can not stand close examination, Professor Rettger arrives at the conclusion

> He makes more assumptions than are usually made in elementary text-books of mechanics.

Let us consider the main points of his criticisms in detail and see whether the foregoing statement is based upon facts.

> On page 16, he introduces the conception of ''force'' as an ''action'' and without hesitation applies vector addition to a system of forces. What is he doing here, but assuming the ''parallelogram of forces'' in its most general form?

It is intimated here that the "parallelogram of forces" is a dynamical law which I have "assumed" without formally introducing it as a new law. It is a fact that I have applied vector addition to forces "without hesitation," but I have shown as little hesitation in treating velocities, accelerations, torques, linear momenta and angular momenta as vectors. Why did not Professor Rettger accuse me of having assumed the "parallelograms" of these magnitudes? Is the "parallelogram of forces" more of a dynamical law than the "parallelogram" of torques, for instance? The "parallelogram" law applies to any vector and is not at all a characteristic of forces, therefore it is not a dynamical law. It does not even deserve being called a "law" when applied to a special type of vectors. In its most general form the "parallelogram law" is the principle of the independence of mutually perpendicular directions in space, a purely geometrical principle. A special case of it is known to students of plane trigonometry as the "law of cosines." In the first chapter of my book this principle is given in its most general form as well as in its several special forms, and is applied to vector magnitudes of different types. After devoting an entire chapter to vector addition and after defining force as a vector, to introduce the "paralellogram of forces" as a new law, as Professor Rettger would have it, could serve only to show that the man who did it could not have a clear conception of the meanings of the terms he was using.

> On page 102 he assumes that a force is proportional to the acceleration produced. This assumes Newton's second law.

This statement is not quite right. The relation between force and acceleration, which I have called *force-equation,* is derived on page 106 from the fundamental principle which I have postulated. In this derivation I have made use of the definition of *kinetic reaction,* which is stated and illustrated on pages 102 to 105, but this is not equivalent to "assuming" a new principle. Will Professor Rettger claim that to define the terms used in a principle is equivalent to introducing or "assuming" new principles? Suppose I had based my work upon the principles of the conservation of energy and of the conservation of momentum should I have no right to classify and define the different forms of energy and of momentum without being rightly accused of having introduced new principles? Will Professor Rettger consider the definitions of momentum, of potential energy, and of kinetic

energy as principles and state that mechanics can not be based upon the principles of the conservation of energy and of momentum alone?

What about the law itself? The first part of the law is clear. "To every action there is an equal and opposite reaction" is nothing but Newton's third law of motion. The word "or" leads us to think that the second part means the same thing as the first part.

Had Profesor Rettger examined my book with greater care he would have noticed that I have used the term "reaction" in a slightly different sense and that with this difference the "first part" is not at all Newton's third law but has the same meaning as the "second part," and that the two "parts" are only two different forms of the statics principle. Further he would have seen that the first form is not made use of, the entire work being based upon the second form alone, and would not have charged me of having assumed Newton's third law in addition to the one I have introduced. The first form is left out entirely in the papers which I published on the subject.[1] In one of these papers I have even shown that Newton's third law is a direct consequence of the second form.

I have postulated the following principle, which I have called the *action-principle:*

The sum of all the actions to which a body or a part of a body is subject at any instant vanishes:

$$\Sigma A = 0.$$

Then I have classified and defined the different forms of action. On this principle I have based my treatment of mechanics, and claim that I have given it a degree of unity and logical continuity which is not common to treatments of elementary mechanics. This is made possible by the simplicity and flexibility of the action-principle, which is easily grasped by the beginner, yet conveys a depth of meaning and breadth of application commensurate with the knowledge and ability of the student.

Besides this pedagogical advantage my treatment involves a point of view which is in harmony with our present ideas of dynamical phenomena, as it is shown in my recent paper on the subject.[2]

H. M. Dadourian

Yale University

[1] "On a Progressive Development of the Principles of Mechanics," *Physical Review*, May, 1913; "On a Progressive Development of Mechanics Based Upon a New Form of the Fundamental Principle of the Science," *American Journal of Science*, February, 1914.

[2] *Loc. cit.*

A NEW METHOD OF COOPERATION AMONG UNIVERSITIES

In April, 1910, was formed at Kansas City, Missouri, the Missouri Valley Conference of Heads and Governing Boards of Universities. The Conference embraced, however, only the institutions up to that time belonging to the Missouri Valley Conference for athletic purposes. There have been many conferences and associations of professors of universities and presidents of universities, or both, and there have been conferences of school boards representing the public schools in various cities, but this is probably the first attempt on the part of university governing boards to accomplish a general understanding and cooperation in regard to matters affecting institutions similarly situated. The conference arose over the matter of intercollegiate football, the question so fruitful of controversy and discussion. The reason for the conference was as follows: There had been introduced into the board of regents of the University of Kansas a resolution abolishing intercollegiate football. The vote was a tie and the motion was lost. The question was brought up again and after thorough discussion it was agreed by the board of regents of the University of Kansas that it was unwise to attempt to settle that question in one university alone and that all of the universities of the then existing Missouri Valley Conference, through their heads and governing boards, should be asked to meet in a general conference at Kansas City in April, 1910. It was at the time of large and pointed discussion and criticism of intercollegiate football and after the matter had been clearly laid before the institutions most interested all of them accepted the invi-

tation and all sent delegates except the University of Iowa. The institutions represented were the University of Missouri, the University of Nebraska, Washington University, Drake University, the Iowa State College, and the University of Kansas. Of these the universities of Nebraska, Missouri and Kansas were represented by members of the board of regents or curators and the presidents of the institutions. Drake University was represented by its president, Iowa State College of Agriculture and Washington University by professors sent by the governing boards of the institutions to represent them. The meeting resulted in a general conference upon athletics as affecting institutions in the Missouri Valley and rules were passed by the Conference and afterwards reenacted by the individual boards of regents, largely affecting the status of intercollegiate football. Among these was the rule abolishing the game on Thanksgiving Day, abolishing the short-term professional coach, and requiring that all college games be played on college grounds.

The second conference was held at Des Moines, January 6, 1911, at which various questions left over from the Kansas City meeting were discussed and acted upon. At that conference the University of Iowa was also represented by its president and board of regents. Washington University was not represented. The discussion at this conference widened out to include other things than athletics. A general discussion of the fraternity question was ordered for the next meeting and committees on uniform financial accounting and uniform pedagogical accounting were authorized. It was plain from the discussions at the second conference, and indeed by formal action, that it was intended to make the conference a permanent one to take into consideration any question touching the common life of universities that might need consideration and uniform action.

The third meeting of the Conference was held in Lincoln, Nebraska, January 19, 1914. The University of Iowa had in the meantime withdrawn from the Missouri Valley Conference and the State Agricultural College of Kansas had been added. All of the institutions in the Conference were represented. Most of the attention of this conference was given up to matters other than athletic and it was more evident than before that the Conference was developing into a general conference on the welfare of the universities having so much in common. The fraternity question received much attention, as did the question of competency in teaching. It is probable that in succeeding meetings such questions as the following may be taken up and discussed, if not formally acted upon: the ethics to be observed in calling teachers from one institution to another; substantially uniform salaries for the same grade of instructors; cooperation in giving advanced and little called for courses; interchange of students and instructors; cost of education. It seems possible, therefore, that this Conference is a beginning of a new type of cooperation, having especial significance and authority because of the fact that the Conference is made up of presidents and governing boards where the primary power lies.

FRANK STRONG,
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UNIVERSITY OF KANSAS

SCIENTIFIC BOOKS

From the Letter Files of S. W. Johnson. Edited by his daughter, ELIZABETH H. OSBORNE. Yale University Press. 1913. Pp. 292.

A notable feature of the applications of science to the arts and industries which characterized the second half of the nineteenth century was the phenomenal evolution of agencies for scientific investigation in the interest of agriculture and the rise of a system of public research institutions extending over every country of the civilized world. The life story of the subject of this biography is essentially the story of the birth of this system in the United States and its growth from a few modest analytical laboratories to an imposing group of national and state institutions actively engaged in agricultural research, in the teaching of agricultural science,

and in the dissemination of the results of investigation.

The career of Samuel W. Johnson presents few dramatic features. He lived the quiet, simple life of the student, occupying a professorial chair in a single university for forty years, yet few, if any, have exerted a more profound influence for the promotion of scientific agriculture. A man of genius as well as of thorough training, he early conceived the idea of making the conquests of science serviceable to the basal industry of the country. Even in his student days, in 1851, he published an article—"County Agricultural Institutes"—setting forth his earliest conceptions of the ideas which later assumed a more definite form. Five years later, in an address before the New York State Agricultural Society on the subject "The Relations which exist between Science and Agriculture" he said:

"I have full faith not only that science *may* accomplish much for agriculture in the way I have indicated, but that she *will* be speedily put about the work. The tendencies of our time prophesy this. The notion that there is anything essentially antagonistic between science and practise is daily meeting its refutation, both in the laboratory and in the field. I may confidently ask, where better than in our own country shall this idea find realization? Our country now has the strength of the oldest nations with all the freshness of youth. She is girding herself up to contest among the nations for the prize of science. What worthier triumph for our republic than to win for her millions the boon of a rational agriculture?"

But Professor Johnson had not only the genius to conceive this ideal and the faith to follow it throughout a long and fruitful career, but the tact and persistence necessary to bring about its institutional embodiment.

His first opportunity presented itself in connection with the introduction of commercial fertilizers into the United States. In March, 1853, he published, under the title "Superphosphate of Lime," an account of the results of analyses which he had made of two samples of artificial fertilizers offered for sale. This work was probably the first of its kind in this country and was the prototype of a vast amount of similar work during the next twenty-five years, done at first as a private undertaking and later as chemist of the Connecticut State Agricultural Society and of the Connecticut State Board of Agriculture. "It was characteristic of the man first to form and tenaciously hold the broad idea, based upon a universal and permanent need; and then, realizing an opportunity for practical work, to set about using his skill and knowledge in routine analysis performed with all possible accuracy in order that these simple analyses should be so absolutely right that they might be an unassailable foundation for the wider work to come after."

In 1853 he was appointed first assistant and in 1856, professor of analytical chemistry in the Yale Scientific School—later the Sheffield Scientific School—and with various titles remained an active member of its faculty until 1896. During all these years, with the capacity and equipment to take high rank among scientific investigators, he devoted his powers chiefly to the instruction of his students, to the preparation of those classic text-books, "How Crops Grow" and "How Crops Feed," and to the service of the farmers of his state in promoting the popular understanding of the aid which science could render to agriculture. His platform was the farmers' meeting, his means of publication chiefly the official report and the agricultural newspaper; while the humble and prosaic work of fertilizer analysis served to furnish the practical demonstration.

Not for almost a quarter of a century did he see the concrete result of his labors in the establishment in Connecticut of the first agricultural experiment station in the United States, at first as a semi-private institution and two years later as an independent state institution under his directorship. This was followed by the founding of similar stations in other states in rapid succession, culminating ten years later in the passage of the "Hatch

Act," providing national support for at least one such station in every state. At the time of his death, in 1909, there were fifty-six of these stations in the United States with an average annual income almost eleven times that of the Connecticut station at its foundation, to say nothing of the enormously increased research activities of the United States Department of Agriculture. Truly the little seed planted in 1853 had become a tree.

In the organization and development of these new institutions the standards established by Professor Johnson and the experience gained at the Connecticut station were material factors in bringing about the success which was so soon attained. At the outset, the American stations were of necessity largely occupied with the analysis and valuation of fertilizers. From the very start, however, original research formed a part of the program of the Connecticut station, while the increase of the state appropriation in 1882 and the assignment to the station in 1887 of part of the Hatch Fund, enabled investigation to be extended to wider fields. Throughout, the work of this station, both under Professor Johnson's administration and that of his successor, has been characterized by the same sane method, the same absence of sensationalism and the same confidence in the power of good works which characterized the fertilizer analyses of the early fifties.

In 1896, Professor Johnson became professor emeritus, and in 1900 resigned the office of director of the experiment station, occupying for a year longer the position of advising chemist which was created for him. He passed peacefully away July 21, 1909, having retained to the last his keen interest in the progress of science and in the problems presented in the development of modern chemistry.

Such was, in barest outline, the active life of an unusually gifted man who had a high conception of the obligations of the scientist to the public. No brief review can do justice to the delightful personality of the man as those knew it who were closely associated with him, and which pervades the book like an aroma, revealing itself especially in the judiciously chosen extracts from his correspondence which constitute the major portion of the volume. His biographer has done her work, not only with filial piety but with notable discrimination and restraint and with marked literary ability. In these days of intense emphasis upon the practical, no more inspiring or elevating volume can be recommended to the student of agriculture who is looking forward to a career as teacher or investigator than this record of a life which attained success in the best sense through the unselfish consecration to the public service of the rigid training and high ideals of the genuine man of science.

H. P. ARMSBY

STATE COLLEGE, PA.

Researches on Irritability of Plants. By J. C. BOSE. London and New York, Longmans, Green and Co. 1913. Cloth, 15 x 23 cm. Pages xxiv + 376; 190 illustrations, largely graphs. Price $2.50.

Physiologists who are familiar with the earlier electrophysiological researches of Bose will be interested in his recent volume on certain kinds of plant responses, which recounts the results of an application of his very ingenious methods to new kinds of problems. Research workers will find this book replete with novel ideas and novel ways of attaining quantitatively comparable measures of plant irritability. The author is not primarily dealing with the fundamental problems of protoplasmic phenomena; his work may be said to concern itself, rather, with the physics of the plant as a whole, or with that of its organs, than with the component cell happenings to which recent physiological inquiry seeks to reduce these aggregates. It is somewhat remarkable that animal physiology, on the one hand, has attained a high state of development along the lines here dealt with (with its studies of the superficial phenomena of muscle contraction, blood pressure, the electrophysiology of muscle and nerve, etc.), and that the findings of this sort of study form a very

considerable basis for the more fundamental researches that prevail at the present time, while plant physiology, on the other hand, has come to the study of cell phenomena largely without passing through this stage. Electrophysiology plays but a very subordinate rôle in plant physiological texts, and refined quantitative studies of the grosser movements of the plant have never had great vogue. It is in just this general field that Bose's writings lie, and his results, while not calculated now to arouse great interest *per se*, must be taken into serious account and will doubtless throw valuable indirect light upon the more fundamental lines of study now mainly attracting attention.

The author makes, at the very beginning of the book before us, a suggestion which has great possibilities in physiological research. "Is there any means by which we might find out whether a given influence has contributed to the plant's well-being or the reverse, whether it has left it more or less excitable," etc.? "The relation between the stimulus and the response would thus form a gauge of the physiological condition of the organism" (pages 1 and 2). In physiological terms, the *tone* of the organism as a whole is here in question, and the quantitative relations between stimuli and response are to be drawn upon as measurements of tone. Now, it is just this matter of the physiological condition of organisms for the estimation of which we most require objective methods; it has been the practise of students of plant physiology to define their organisms in the terms of the older descriptive work, assuming, for example, that a number of given plants are the same thing physiologically because they exhibit the same superficial shapes, sizes, etc., to the taxonomist. Two workers may employ the same species or variety in the taxonomic sense, but their organisms may be very different physiologically and they may disagree entirely on the sort of response attained with a given stimulus or set of environmental conditions. If some of the dimensions or characteristics of the internal condition of the organism used in an experiment could be measured and stated, much would be done to avoid many of the wasteful arguments which so often diminish the efficiency of physiological workers. From Bose's suggestion it appears to the reviewer that the methods of the present work may be of value in the physiological definition of our plant subjects, just as similar methods in animal physiology are proving of value to medical diagnosticians.

The subject-matter of this book defies useful treatment in a review. A few statements may, nevertheless, be made here. Most of the study has to do with the familiar paratonic movement of the leaf of Mimosa, though other organs receive attention at particular points. To obtain an automatic graph ("phytogram" or "plant script") of this response, a thread is attached to the petiole and to a light bent lever above, the latter counterbalanced and carrying a writing point which moves over a smoked surface. A glass plate, allowed to fall by clock work, furnishes the receptive part of the apparatus. To avoid too great friction it was found necessary to make the recording point vibrate in a plane at right angles to the receiving plate, and it was possible to arrange the vibrations so as to mark time. Thus a record with this "resonant recorder" appears as a line indicated only by a series of dots on the smoked surface, the distances between the dots indicating small units of time. The apparatus is so ingenious and delicately efficient as to excite wonder and admiration in and for itself. Stimuli of several kinds are employed, applied to the leaf in various ways. The most frequently used are thermal (obtained electrically, so as to be quantitatively controlled) and electric. The graphs show a short latent period, a period elapsing between beginning and end of the fall of the leaf ("apex time") and a long recovery period. Besides these three time periods is of course to be considered the amplitude of the movement, in characterizing the nature and intensity of the response.

Some of the topics experimentally dealt with are the following—the terms themselves show the parallelism between the plant phenomena here described and the familiar ones

called by the same names in animal physiology: additive effects; influence of load, temperature and intensity of stimulus; fatigue; staircase response (quite like that of muscle); tetanization; death spasm (it looks as though a means were here offered for determining when death of a tissue ensues); influence of gases on excitability; effect of intensity of stimulus, of fatigue and of temperature on latent period; effect of various conditions on velocity of transmitted impulse; positive and negative galvanometric and turgidity responses (each stimulus gives rise to both, but the weaker positive—erectile—response is quickly followed and masked by the stronger negative—"contractile"—one); polar effects, etc. Polar effects are very thoroughly investigated. "With feeble current the kathode excites at make and not at break. The anode excites at neither make nor break," etc. The variation of polar reaction under tissue modification, as with age, etc., is studied, with important results. In the later chapters of the book *Biophytum* and *Desmodium gyrans* are employed in studying multiple and automatic (autonomic) responses. The pulsations of *Desmodium* leaves are thoroughly investigated.

The first two sentences of the last paragraph of Bose's book sum up the general outcome of his studies as well as can be thus briefly done: "In surveying the response of living tissues we find that there is hardly any phenomenon of irritability observed in the animal which is not also found in the plant. The various manifestations of irritability in the plant have been shown to be identical with those in the animal."

B. E. LIVINGSTON
JOHNS HOPKINS UNIVERSITY

SOCIETIES AND ACADEMIES

ANNUAL CONVENTION OF THE UTAH ACADEMY OF SCIENCES

THE seventh annual convention of the Utah Academy of Sciences was held at Salt Lake City, December 26 and 27, 1913, in the chemistry lecture room of the University of Utah.

In all, three sessions were held—the first beginning at 8 o'clock Friday evening, the second at 9:30 o'clock Saturday morning, and the closing session at 2 o'clock Saturday afternoon.

From the standpoint of business transacted, this convention of the academy will rank as one of the most important in its history. A committee was appointed to take steps toward the publication of a bi-monthly magazine under the auspices of the academy. Another committee was appointed to look after the entertainment of distinguished scientists who may be visiting in Salt Lake City, and to assist them should they desire to investigate features of the region of scientific interest in their especial lines.

Professor C. W. Porter, of the Utah Agricultural College, was elected to fellowship. Miss Florence Knox, Professor Christian D. Steiner, Professor Jakob Bolin, Professor Franklin O. Smith, Dr. Helen I. Mattill, all of the University of Utah; Dr. Fred W. Taylor and Mrs. Amelia R. Taylor, of Provo; Willard R. Harwood and N. W. Cummings, of Salt Lake City; Professor George R. Hill, Utah Agricultural College, Logan, and H. R. Hagan, Logan, were elected to membership.

The constitution was amended to provide for a permanent secretary-treasurer.

The following are the officers for the ensuing year:

President—Professor Marcus E. Jones, Salt Lake City.

First Vice-president—Dr. Harvey Fletcher, B. Y. U., Provo.

Second Vice-president—Dr. C. N. Jensen, B. Y. C., Logan.

Permanent Secretary-treasurer—A. O. Garrett, High School, Salt Lake City.

Councillors-at-large—Dr. A. A. Knowlton, U. U., Salt Lake City; Dr. Joseph Peterson, U. U., Salt Lake City, and Dr. F. L. West, U. A. C., Logan.

The following papers were read at the annual convention:

"The Question of Valency in Gaseous Ionization," by Dr. Harvey Fletcher, B. Y. U., Provo.

"Community Life among Insects" (the presidential address), by Dr. E. G. Titus, U. A. C., Logan.

"Some Metabolic Effects of Bathing in Great Salt Lake," by Dr. Helen I. and Dr. H. A. Mattill, U. U., Salt Lake City.

"Corn under Irrigation," by Dr. F. S. Harris, U. A. C., Logan.

"Practical Experiments with Root-borers," by

Professor M. Rich Porter, Weber Stake Academy, Ogden.

"Workable Phosphates of the Mississippian," by Professor William Peterson, U. A. C., Logan.

"How Far is Scientific Extension Practicable?" by Dr. E. G. Peterson, U. A. C., Logan.

"Some Features of the Recent International Congress of Geologists," by W. D. Neal, Salt Lake City.

"Outline of Mining and Smelting Conditions at Santa Fe., N. M.," by B. A. Berryman, San Pedro, N. M.

"The First Law of Irrigation Practise," by Dr. John A. Widtsoe, U. A. C. (Read by title.)

"Uranium and Vanadium Deposits in Utah," by Professor Marcus E. Jones, Salt Lake City.

A. O. Garrett,
Secretary

New Orleans Academy of Science

The regular monthly meeting of the Academy was held in the Richardson Memorial Building, Tulane University, on Tuesday, February 17, with Dr. Isadore Dyer, president, in the chair and a large number of fellows and members present. The president appointed a committee of two to draw up suitable resolutions upon the death of Dr. Alcee Fortier, who had been a fellow of the Academy for thirty years and for some time corresponding secretary. The first paper of the evening was read by Dr. Irving Hardesty, professor of anatomy, Tulane University.

A number of objections to the Helmholtz theory of hearing were cited and attention was called to the impossibility, anatomically, of the basilar membrane serving in the manner usually assumed for it. Then the tectorial membrane was shown to be coextensively with the organ of Corti, to occupy the logical position, extending with one edge free over the peripheral surface of the neuro-epithelium and nearer the scala vestibuli, and that it is far more adapted and capable of being the chief vibratory structure in hearing, its flexibility being much greater and its proportions varying far more than those of the basilar membrane. A telephone theory of hearing was applied to the tectorial membrane and a model was shown in which the three divisions of the auditory organ were represented and in which the tectorial membrane was simulated as nearly as possible, both as to position and varying proportions. Sounds of varying vibration frequently applied to this model indicated (1) that the tectorial membrane does vibrate; (2) that sounds of low vibration frequency throw the entire membrane into vibration; (3) that sounds of high frequency throw only varying extents of the more slender, basal end of the membrane into vibration, such sound waves being so damped out before reaching the apical end of the cochlea as to be incapable of producing vibrations in the thicker, apical end of the membrane; (4) a certain small amount of resonance seems apparent in the tectorial membrane, but probably little more than is possessed by the diaphragm of the telephone.

The second paper was by Dr. R. B. Bean, also of the department of anatomy, Tulane University, on "The Cartilaginous Tip (Woolner-Darwin) and the Skin Tip of the Human Ear." The speaker explained that "Darwin's tubercle" in the adult represents the cartilaginous tip of the ear of the fetus, which turns forward during the late stages of fetal life. There is at the same time a folding over of the skin and a shrinking with the folding, which leaves lines in the skin over the upper outer part of the helix. These lines are present on all ears that have been observed so far. They converge ventrally, thus indicating where the skin tip has turned under the ventral edge of the helix. The lines are not always over "Darwin's tubercle" but are frequently superior to this cartilaginous point, which seems to indicate that the skin or cartilage has shifted in development. By means of these lines one is able to demonstrate that certain distorted ears, which look as if they had been injured by accident, are in reality normal. The majority of the ears of about 150 negroes preserved in formalin at Tulane University seem to be distorted, and until these lines were discovered they were considered abnormal, frost-bitten, scarred, or otherwise mutilated, but they appear to be normal because the lines of the skin are present in their proper position and relations. Observations upon thousands of negroes show that this condition of apparent distortion of the ear is characteristic of that people. The ear of the Negro may, therefore, be called a distorted or mutilated looking ear. It is smaller than the ear of the white people, and shows a greater extent of retrograde metamorphosis.

There was considerable discussion of both papers in which Dr. Mann, Dr. Clo, Dr. Dyer and others participated. The academy then adjourned.

R. S. Cocks,
Secretary